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CONTENTS

ARTICLES

One Soul at a Time: Political Science and Political Reform
Samuel P. Huntington 3

Structural Dependence of the State on Capital
Adam Przeworski & Michael Wallerstein 11

Max Weber's Liberalism for a Nietzschean World
Mark Warren 31

Political Science and the Enlightenment of Enthusiasm
James Farr 51

American Political Science, Liberalism, and the Invention of Political Theory
John G. Gunnell 71

Political Jurisprudence, the "New Institutionalism," and the Future of Public Law
Rogers M. Smith 89

Interest Group Politics and Economic Growth in the U.S. States
Virginia Gray & David Lowery 109

Institutions, the Economy, and the Dynamics of State Elections
John E. Chubb 133

Nuclear Brinkmanship with Two-Sided Incomplete Information
Robert Powell 155

Crisis Learning Games
Russell J. Leng 179

The Core and the Stability of Group Choice in Spatial Voting Games
Norman Schofield, Bernard Grofman, & Scott L. Feld 195

Principals, Bureaucrats, and Responsiveness in Clean Air Enforcements
B. Dan Wood 213

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CONTROVERSY

Do U.S. Senators Moderate Strategically?
Robert A. Bernstein, Gerald C. Wright, Jr., & Michael Berkman 237

BOOK REVIEWS

Review Essays 253
Political Theory 261
American Politics 282
Comparative Politics 309
International Relations 335
Political Economy 351

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MARCH 1988
VOLUME 82 NUMBER 1

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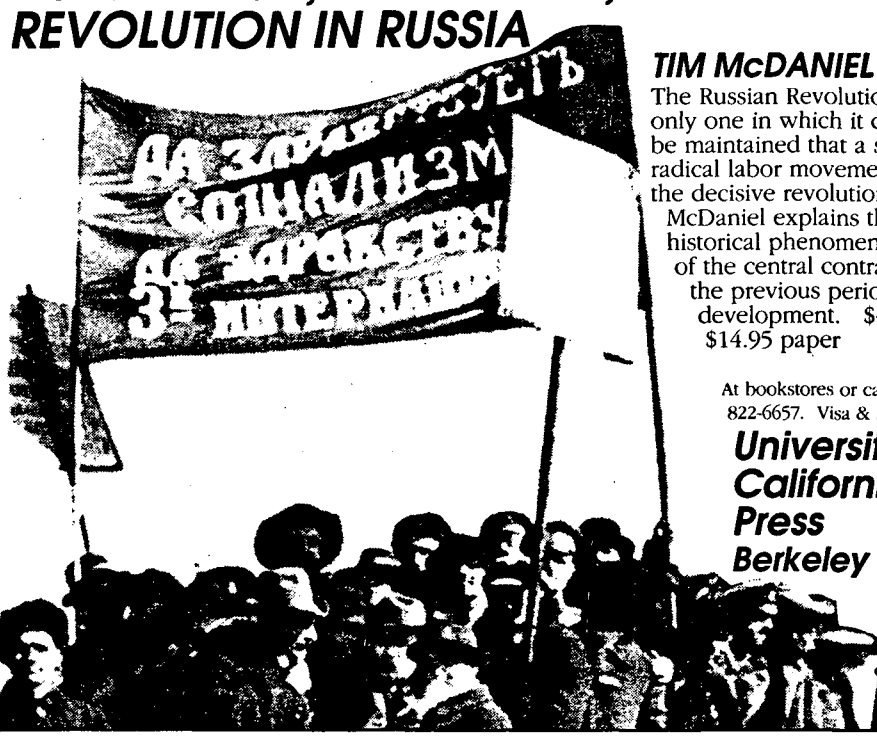

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ONE SOUL AT A TIME: POLITICAL SCIENCE AND POLITICAL REFORM

SAMUEL P. HUNTINGTON
Harvard University

Political scientists want to do good. They want to expand knowledge about political life, but also they wish to use knowledge for political reform. Usually this means desiring to promote "democratization." Historically democracy and political science have tended to develop together. In modest ways political science can contribute to the emergence of democracy. Political reform succeeds best if it occurs incrementally, in the spirit of "one soul at a time."

Let me begin with what may be an unsettling and perhaps even shocking observation about our profession but one that I derive from forty-four years of watching political scientists at work: by and large, political scientists want to do good. To mitigate the impact of this statement, let me quickly add two additional points. First, because political scientists want to do good does not mean that they actually do good. They may fail miserably to realize their intentions, although that does not mean that they therefore do evil. Second, by *want to do good* I do not mean that political scientists simply want to expand our understanding of politics. Political scientists want to do that: that is the professional calling of political science. The expansion of knowledge is, of course, a good in itself and may be pursued as such. What I am suggesting is that political scientists want to expand our knowledge of politics because they usually see or sense a link between this expanded knowledge and broader social goals or public purposes. The exact nature of these purposes varies, of course, among members of the profession. Prominent among them are the promotion of justice, well-being, order, equity, liberty, democracy, responsible government, security for individuals and states, accommodation among groups, peace among nations. In some measure these goals may be subsumed under the broad category *political goods* that Roland Pennock elaborated over two decades ago (1966, 415–34).

In particular circumstances these goals may conflict with each other. Few people, however, would deny ultimate legitimacy to any of them, even if they disagreed vigorously over their relative priority in situations where choices have to be made. It is hard, indeed, to think of a major work in political science that is not inspired in some measure—perhaps remotely but often very directly—by one of these purposes. Political science, in short, is not just an intellectual discipline; it is also a moral one. Morality, observes Albert Hirschman (who, I might note, was elected most deservedly although belatedly to the National Academy of Sciences last spring), "belongs into the center of our work; and it can get there only if the social scientists are morally alive and make themselves vulnerable to moral concerns—then they will produce morally significant works, consciously or other-

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wise" (1981, 305). From this it follows that works in the social sciences should be judged not only on their intellectual merit but also by the contributions they make to achieving moral purposes. We may intellectually admire "neat" and highly sophisticated analyses, quantitative or otherwise, of esoteric scholarly issues; but implicitly we do not accord them the same status we do the works of Dahl probing the preconditions of democracy, Walzer grappling with the nature of just war, Verba illuminating the dimensions of equality, or Horowitz exploring the ways of resolving ethnic conflict.

The phrase *doing good* is rather sweeping in scope, in that it could encompass all sorts of personal and public goals, political and otherwise. It also conveys a somewhat mushy impression. Let me, therefore, dispense with that phrase and instead use one more directly emphasizing doing good in politics, that is, political reform. By *political reform* I mean the peaceful—hence gradual, hence usually negotiated—enhancement of liberty, justice, equality, democracy, and responsibility in politics. The impetus to do good in the sense of promoting political reform is, I would argue, embedded in our profession.

The historical relation between political science and political reform, it should be noted, has a very specific meaning in the American context. The emergence of political science was "part of a broad movement of Progressive reform in American intellectual and political life" at the end of the nineteenth century. The major figures in political science, among them A. Lawrence Lowell, Woodrow Wilson, Frank Goodnow, Albert Bushnell Hart, and Charles Beard, were associated with the Progressive movement (Huntington 1974, 7). "To call the roll of the distinguished social scientists of the Progressive era," Richard Hofstadter noted, "is to read a list of men prominent in their criticism of vested interests or in their support for reform causes" (quoted in Huntington 1974, 5).

This particular historical association between political science and political reform rests on a logical basis. Political science, as Lowell and Wilson emphasized, is or should be devoted to studying the realities of politics, the how and the why of political behavior. This heavy emphasis on the empirical exploration of the realities of politics tends to drive political scientists in two directions. First, politics turns out to be extremely complex and ambiguous; hence there are not only no easy solutions but also generally speaking no simple or obvious solutions to political problems. The nature of politics tends to reinforce nonideological, nondoctrinaire, and pragmatic tendencies among those who study it. Now obviously I do not want to push this argument too far. We all know political scientists—much-admired political scientists—who are ideological if not doctrinaire and who have advanced simple, if not simplistic, remedies to complex problems. I am arguing only that the logic of political inquiry works in the opposite direction, not that everyone is necessarily persuaded by that logic.

Second, while the exploration of political reality—understanding things as they are and why they are as they are—may push political scientists in a conservative direction, it also tends to leave them in a dissatisfied mood. Knowing how things work in practice can only remind one of the large gap between that and how they ought to work in theory. As political scientists quickly discover, the behavior of politicians, bureaucrats, and voters and the operations of political institutions and processes often leave much to be desired. In a sense, political scientists suffer an intense disciplinary version of what on a broader scale I once called the *IvI gap*. And a not unnatural response to this gap on the part of political scientists is, implicitly or explicitly, to suggest ways—

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usually modest and occasionally practical —for reforming things. Quincy Wright once said that after a "general education in political science . . . it is hard to see how a political scientist can be either a Nazi or a Communist" (quoted in Ricci 1984, 170). It is also hard to see how a political scientist can be entirely happy with the status quo.

The study of political science thus tends to highlight both what's wrong with politics and how difficult, complex, tedious, ambiguous, and uncertain it is to correct what's wrong. Political scientists come to learn only too well the truth behind Oxenstierna's famous lament, "If you only knew, my son, with what little wisdom the world is governed!" But they come to learn equally well the familiar biblical truth that "the children of this world are in their generation wiser than the children of light." The problem of combining both truths, both perspectives, is perhaps most dramatic in our teaching. How do we make our students wise about politics without also making them disillusioned with politics?

Despite the efforts of political scientists, wisdom about politics is in short supply in today's world. Much of modern politics is, on the surface, abolitionist politics. It is devoted to efforts to do away with things: abolish war, abolish nuclear weapons, abolish injustice, abolish tyranny, abolish apartheid. Those demanding fundamental change typically describe the existing political system as the epitome of evil, the worst system possible. Hence all contact or cooperation with it must be avoided. Hence it must be totally destroyed. This is a simple and effective political strategy. Focusing on an evil, whose evilness cannot be denied, generates moral outrage, facilitates political mobilization, and sets forth a clear, simple, and unifying goal, while avoiding the divisive issue of what should replace that evil. If the existing political system is the worst possible system, then of course virtually any means, any amount of violence, is legitimate in attempting to overthrow it.

The study of politics, however, shows clearly that the process of doing good involves far more than simply doing away with evil. It is difficult to imagine worse political systems than those of Hitler or Stalin. Yet however bad a given evil may be, a worse one is always possible and often likely: witness the unhappy recent experiences of Cuba, Vietnam, Cambodia, Ethiopia, Nicaragua, and Iran, among others. The political scientist, consequently, can only be skeptical of a claim that, as one Chilean dissident put it, "there is no way in which we are going to see anything worse than General Pinochet" or the argument of an opponent of apartheid that "the first African government of South Africa . . . could scarcely be any worse" than the current regime. And let me hastily add, lest this be read as a political statement, that I can easily conceive of worse systems than that in the Soviet Union, even before Gorbachev began to change it. Fifty years ago, indeed, the Soviet system was much worse than it is now. Military authoritarianism, racial oligarchy, and Communist dictatorship are evil political systems; but the assumption that things could not become worse than they are in Chile, South Africa, or the Soviet Union defies logic and history.

Political reformers wise in the ways of politics understand this and hence tend to be leery of simple solutions and of revolution and revolutionaries. This skepticism has been well expressed in the statements of three notable contemporary democratic reformers from three different continents, one unsuccessful, one successful posthumously, and one as yet neither successful nor unsuccessful. First, "We know lots of revolutions, great revolutions, and magnificent people," said Lech Walesa, "who after taking over power, produced systems that were much worse than the

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ones they destroyed. We do not want to make this mistake." Second, "In a revolution," said Benigno Aquino in a speech drafted for delivery at Manila Airport, "there can really be no victors, only victims. We do not have to destroy in order to build." Third, "Bloody revolutions," Mangosutho Buthelezi warned, "fought against terrible oppression do not automatically bring about great improvements."[2]

An extremely close relationship exists between political science and one particular type of political reform. Robert Heilbroner once noted that it is impossible to have economists in the absence of commercial relations and that it was the development of the market system in early modern times that, in his phrase, "called forth the economists." Had Adam Smith lived in an earlier age, Heilbroner says, he "might have been a great moral philosopher," but "he could never have been a great economist; there would have been nothing for him to do" (1980, 18, 27). Somewhat similarly, it can be argued that it is impossible to have political scientists in the absence of political participation, and political science has only developed with the expansion of political participation. In a society in which there is no participation—no competition for power—political scientists would have nothing to do. Consequently, it is not surprising that a market propensity tends to prevail among economists and a participatory or democratic propensity among political scientists. Command economies have no use for economists, nor authoritarian politics for political scientists.

The connection between democracy and political science has been a close and continuing one. Nowhere was that connection more meaningful than in the event whose two hundredth anniversary we celebrate this year. The men who gathered in Philadelphia in the summer of 1787 were, in John Roche's felicitous phrase, "a nationalist reform caucus": They were democratic politicians struggling to fashion a pragmatic compromise that would both create a strong national government and be acceptable to the people of the states (1961, 799). As Austin Ranney pointed out on this occasion twelve years ago, Washington, Hamilton, Adams, and Jefferson all stressed the central importance of the study of the "science of politics" and the "science of government" to the work they were engaged in of creating a new nation (1976, 141–43). A gathering at Harvard the year the Constitution was ratified voted that it was "more necessary in a Republic than in any other form of government that young men should be instructed in political science" (William Coolidge Lane, quoted in Crick 1959, 5).

The creation of a republic and the development of democracy called forth political science and political scientists. The relationship is nowhere more notable than in countries, such as Germany and Italy, that, prior to World War II, had strong traditions of scholarship in history, social theory, and sociology but not in political science. As Hans Kastendiek has pointed out in his study of Germany, "Because of the structure of politics in the 19th century and the political power relations which characterized the Weimar Republic and were mirrored in the academic system, conditions for a distinct political science simply did not exist. . . . The emergence of the discipline after 1945 was due to specific political constellations. . . . 'To build up democracy' and to install a distinct political science were needs directly connected to each other" (1987, 26).

Bernard Crick thus got it wrong in the title of his famous book about our discipline. There is not an *American* science of politics; there is a *democratic* science of politics, which developed first and fullest in the United States because the United States was the first and fullest democracy in the modern world. That science is a

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universal science to the extent that democracy is a universal system of government. Where democracy is strong, political science is strong; where democracy is weak, political science is weak. Authoritarian societies may produce and in some cases have produced Nobel Prize-winning physicists, biologists, novelists, and statesmen; they do not produce great political scientists. The emergence of democracy encourages the development of political science, and the development of political science can and has in small ways contributed to the emergence and stabilization of democracy.

Since 1974, dictatorships have given way to democracies in many countries, including Portugal, Spain, Greece, Turkey, Brazil, Argentina, Uruguay, Peru, Ecuador, and the Philippines. A transition may be underway now in the Republic of Korea; modest steps toward broader political participation have taken place in Taiwan. In still other countries elections seem to be acquiring new popularity. If not reversed, all this bodes well for the future of democracy and for the future of political science. Political scientists are uniquely qualified to study these processes of change, to derive generalizations and lessons from them, and to suggest ways in which this knowledge may be applied to countries where democracy is yet to be realized. Many political scientists, including Linz, Stepan, Lipset, Diamond, O'Donnell, and Schmitter, as well as others, have responded to this challenge and have carried out studies that add importantly to our understanding of the conditions and processes of democratization (see Diamond, Linz, and Lipset n.d.; Linz and Stepan 1978; and O'Donnell, Schmitter, and Whitehead 1986).

I cannot summarize their findings here and I will not attempt to advance any systematic analysis of my own, but I do suggest that the most important lesson from the history of democratization is that what is difficult is not the termination of the nondemocratic regime but the development of stable democratic institutions. In this process political science, if given a chance, may be able to play some small constructive role. Let me illustrate this by reference to three contemporary cases.

Brazil is perhaps the most striking example of democratization introduced from above by a military elite that realized and indeed supported the need to move toward democracy.[3] In 1973 Brazil was a military dictatorship; in 1985 it was a democracy. The genius of the Brazilian transition is that it is virtually impossible to pinpoint the time during the twelve years in between when Brazil stopped being a dictatorship and became a democracy. The Brazilian transition to democracy was in many respects a masterpiece of obfuscated incrementalism. Political science played a modest role in this process. The military leadership that guided the transition was aware of the experiences of other countries and sensitive to the need first to encourage "decompression" (*distensao*) and the "opening" (*abertura*) of their society so as to minimize the probability of extreme polarization and violent upheaval and second to proceed with changes incrementally, often on a two-steps-forward-one-step-backward basis and thus to make democratization, as President Geisel put it, "slow, gradual, and sure." The leading military figure guiding the democratization, General Golbery, was a long-term member of the American Political Science Association; he and other key figures in the democratization process solicited the advice of political scientists; and a younger generation of Brazilian political scientists—who, thanks to the Ford Foundation, had been trained during the 1960s at Stanford, UCLA, Harvard, MIT, Michigan, and elsewhere—played active roles in developing and articulating ideas that were central to the Brazilian process.

For several years China has been in the midst of economic reforms, some of

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which are quite far-reaching. These changes naturally raise the question as to whether equally meaningful political reforms will follow. The centralized authoritarianism of the Chinese political system has, however, had its correlate in the undeveloped state of Chinese political science. Major universities do not have political science departments as such; what is termed *political science* has so far been generally limited to the study of foreign societies and public administration. Kent Morrison quotes one of his graduate students at Zhongshan University in Guangchou as saying in 1984, "To be a political scientist in China is, I think, very dangerous" (Morrison and Thompson 1985, 1). The student is right and the reason is that ultimately, political science can only be very dangerous to the regime in China. Yet the Chinese government is now sending scores if not hundreds of graduate students to the United States to study political science. Inevitably, these students also learn democracy. One can only wonder what the Chinese leaders expect to achieve by doing this. It seems very likely that the leaders either will have to accept a massive brain drain from their society; or will have to find room in their society for a highly intelligent, articulate, and well-connected group of scholarly lobbyists pushing for truly meaningful democratization; or will have to expand their prison system. (In the Soviet Union, it should be noted, what is termed *political science* remains in a primitive state. The work of Soviet political scientists has, however, been closely associated with efforts to reform the Soviet system and to broaden participation in it (Hill 1980; also Brown 1986, esp. pp. 466–69). One test of the meaningfulness of Gorbachev's commitment to *glasnost* and democratization will be the extent to which Soviet scholars are permitted to develop a truly empirical political science, to broaden their contacts with Western political scientists, and to import elements of Western political science into their research and teaching.

Perhaps nowhere in the world is political science more relevant today than in South Africa. Much of the debate over the South African future has been conducted in terms of concepts and hypotheses from political science.[4] Political scientists—white political scientists—have been in the vanguard of those working for the end of apartheid and some form of meaningful sharing of power among all racial groups. What has been most dramatically and tragically missing from the debate have been the voices of black political scientists. The reason is that almost none exist. Yet surely black political scientists should and could make a major contribution to the future political development of their country. Recognizing this need, last year the council of our association advanced a proposal, modeled in part on Ford's 1960s program for Brazil, to bring nonwhite South Africans to U.S. universities for graduate work in political science. I am happy to say that the Ford Foundation will provide a substantial grant to get this program started in the coming year.

Deng Xsao Ping and Mikhail Gorbachev may be reformers but clearly neither has any intention of abandoning the Communist party's monopoly of power in his country. Similarly, P. W. Botha has made significant reforms in the South African system of apartheid. These reforms have, however, been followed by intensified and at times vicious repetition; and there are no signs of any intention to relax the National party's grip on power. In all three countries the intent of the reforms is to strengthen the existing system, not to change it. Under these circumstances, the question then becomes, How can democratic reformers exploit the opportunities offered by preservative reforms to promote more meaningful change? One notable historical case where an exercise in preservative reform was made over into an exercise in fundamental

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reform was the constitutional convention of 1787. That process has perhaps been most closely approximated recently in the 1986 constitution-making efforts that occurred in South Africa.

For eight months, over fifty representatives of the government of the South African province of Natal, the government of the Zulu homeland of KwaZulu, and over thirty other organizations representing all South African racial groups met in Durban and hammered out a constitution for a racially integrated, democratic system of government for that region of South Africa. As in Philadelphia in 1787, provisions for majority rule had to be reconciled with the need to protect minority rights. As in Philadelphia, frequent references were made to the lessons of political science and the experiences of other societies. As in Philadelphia, a few delegates did not sign the final document, but over eighty percent of them did. As with the Philadelphia convention, the Durban Indaba was attacked from both Right and Left (by the revolutionaries of the African National Congress and its front groups on the one hand and by the Afrikaner standpatters on the other). As with the Philadelphia constitution, agreement on the Durban constitution was eased by the anticipation that one widely respected person (in this case Chief Buthelezi) would become the chief executive of the new government. As with the Philadelphia constitution, the Durban constitution also needs to be ratified. In this case, however, ratification means approval not by state assemblies but rather by the national government. To date, that approval has not been denied, but it also has not been forthcoming. It is not clear that even the persuasive logic of a Hamilton, a Madison, and a Jay could bring it about; but it is possible that hard-headed negotiations between the Indaba supporters and the National party may allow some version of the proposed institutions to come into being.

This effort at constitutional change has been virtually ignored in the United States. Closed meetings devoted to the undramatic bargaining and drafting required for political reform do not make good copy for the media; nor do they provide the opportunity, as do police brutality and necklacing, for expressions of moral outrage at the evils of apartheid. The Durban constitution is no overall solution to the traumas of South Africa. Within the context of this one province, however, it is a constructive and hopeful effort to create democratic institutions to replace apartheid. Like the Philadelphia constitution, it is the compromise product of political practitioners informed by political science working to reconcile conflicting interests and principles.

I began these remarks with the suggestion that political scientists, by and large, want to do good and then went on to argue that a close connection exists between political science and political reform and particularly between political science and one type of political reform, that is, democratization. Let me close by emphasizing again the central lesson the study of politics offers to those who wish to bring about political reform.

The Salvation Army has a motto, "Save the world, one soul at a time." Political science has helped, can help, and should help to save the world by generating understanding of political processes, by illuminating the feasibility and consequences of alternative governmental arrangements, and by enhancing appreciation of the potentialities and the limits of political engineering. The most fundamental lesson of the study of politics, however, is that there are no shortcuts to political salvation. If the world is to be saved and stable democratic institutions created, it will be done through *incremental* political reform undertaken by moderate, realistic men and women in the spirit of one-soul-at-a-time. That is the sobering yet hopeful message of our discipline.

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Notes

This essay is the presidential address presented on September 3, 1987 at the 83rd annual meeting of the American Political Science Association in Chicago.

1. Ariel Dorfman, quoted in *Campus Report*, 1986, 1: 8; Pierre L. van den Berghe, quoted in Adam 1971, 118.

2. Lech Walesa, interview with Neal Conan, 5 February 1985, National Public Radio; Benigno Aquino, speech text, *New York Times*, 22 August 1983; Chief Mangosuthu G. Buthelezi, "Disinvestment is Anti-Black," *Wall Street Journal*, 20 February 1985.

3. See, in general, the comprehensive study by Skidmore n.d., esp. chaps. 6, 7.

4. See, for example, Republic of South Africa, esp. chaps. 3 and 4, and the central role that Arend Lijphart's theory of consociational democracy has played in discussions of possible political solutions in South Africa. Lijphart (1985) applies his theory specifically to that country and also provides a comprehensive bibliography of the relevant literature (pp. 137–71).

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Samuel P. Huntington is Eaton Professor of the Science of Government, Department of Government, Harvard University, Cambridge, MA 02138.

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STRUCTURAL DEPENDENCE OF THE STATE ON CAPITAL

ADAM PRZEWORSKI
University of Chicago
MICHAEL WALLERSTEIN
University of California Los Angeles

A central claim of both Marxist and neoclassical political theory is that under capitalism all governments must respect and protect the essential claims of those who own the productive wealth of society. This is the theory of "structural dependence of the state on capital." Using a formal model, the internal logic and the robustness of the theory is examined. We conclude that in a static sense the theory is false: virtually any distribution of consumption between wage earners and owners of capital is compatible with continual private investment once an appropriate set of taxes and transfers is in place. Yet the state may be structurally dependent in a dynamic sense. Policies that, once in place, redistribute income without reducing investment do reduce investment during the period in which they are anticipated but not yet implemented.

Capitalism is a system in which many scarce resources are owned privately, and decisions about allocating them are a private prerogative. Democracy is a system through which people as citizens may express preferences about allocating resources that they do not privately own. Hence the perennial question of political theory and of practical politics concerns the competence of these two systems with regard to each other. Is it possible for governments to control a capitalist economy? In particular, is it possible to steer the economy against the interests and preferences of those who control productive wealth?

The central and only distinctive claim of Marxist political theory is that under capitalism all governments must respect and protect the essential claims of those who own the productive wealth of society. Capitalists are endowed with public power, power which no formal institutions can overcome (Luxemburg 1970; Pashukanis 1951). People may have political rights, they may vote, and governments may pursue popular mandates. But the effective capacity of any government to attain whatever are its goals is circumscribed by the public power of capital. The nature of political forces that come into office does not alter these limits, it is claimed, for they are structural—a characteristic of the system, not of the occupants of governmental positions nor of the winners of elections.

During the past 20 years Marxists have developed several theories to explain why all governments in capitalist societies are bound to act in the interests of capitalists. In one explanation, state managers internalize the goals of capitalists and use the state as an instrument on their behalf (Miliband 1969). Another explanation emphasizes the generic structural and functional limitations: under capitalism, the state cannot organize production (Offe 1975), mandate investment (Lind-

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blom 1977), or command consumption, because these are prerogatives reserved to owners of private property. But the most daring, because the least contingent, theory claims that it does not matter who the state managers are, what they want, and whom they represent. Nor does it matter how the state is organized and what it is legally able or unable to do. Capitalists do not even have to organize and act collectively: it suffices that they blindly pursue narrow, private self-interest to sharply restrict the options of all governments. This is the theory of "structural dependence of the state on capital."

This theory begins with the hypothesis that the entire society depends on the allocation of resources chosen by owners of capital. Investment decisions have public and long-lasting consequences: they determine the future possibilities of production, employment, and consumption for all. Yet they are private decisions. Since every individual and group must consider its future, since future consumption possibilities depend on present investment, and since investment decisions are private, all social groups are constrained in the pursuit of their material interests by the effect of their actions on the willingness of owners of capital to invest, which in turn depends on the profitability of investment. In a capitalist society, the trade-off between present and future consumption for all passes through a trade-off between consumption of those who don't own capital and profits (Przeworski 1985, chap. 5).

Consider this dependence from the point of view of one group, wage earners. At any instant of time, wages and profits are indeed inversely related, as Marx argued in *Wage, Labour, and Capital* (1952a). In a world without a future, wage earners would be best-off consuming the entire product, indeed confiscating the capital stock. But wage earners care about their future as well as present income, and future wages depend on private investment. If firms respond to wage increases with less investment, wage earners may be best-off moderating their wage demands. Workers' future income depends upon the realization of capitalists' present interests.

While the theory is usually stated with regard to workers' wage demands, to the extent that material means are required to advance their welfare, structural dependence binds all groups: minorities struggling for economic equality, women wanting to transform the division of labor within the household, old people searching for material security, workers striving for safer working conditions, or the military seeking bombs. It is in this sense that capitalism is a class society: not in the sense that there are always two organized classes, but in the sense that the structure of property characteristic of capitalism makes everyone's material conditions dependent upon the private decisions of owners of wealth.

The theory of structural dependence continues with the inference that because the entire society depends on the owners of capital, so must the state. Whether particular governments have interests and goals of their own or they act on behalf of a coalition of groups or a class, the pursuit of any objectives that require material resources places governments in the situation of structural dependence. Politicians seeking reelection must anticipate the impact of their policies on the decisions of firms because these decisions affect employment, inflation, and personal income of voters: vote-seeking politicians are dependent on owners of capital because voters are. Even a government that was a perfect agent of wage earners could not and would not behave much differently from one that represents capitalists. If workers are best-off with a fair dose of wage restraint, a prolabor government will also avoid policies that dramatically alter the distribution of income and

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wealth. The range of actions that governments find best for the interests they represent is narrowly circumscribed, whatever these interests may be.

The reason the state is structurally dependent is that no government can simultaneously reduce profits and increase investment. Firms invest as a function of expected returns; policies that transfer income away from owners of capital reduce the rate of return and thus of investment. Governments face a trade-off between distribution and growth, between equality and efficiency. They can trade a more (or less) egalitarian distribution of income for less (or more) investment but they cannot alter the terms of this trade-off: this is the central thesis of the theory of structural dependence. Governments can and do choose between growth and income distribution; but because material welfare of any constituency depends upon economic growth as well as its share of income and because distribution can be achieved only at the cost of growth, all governments end up pursuing policies with limited redistributive effects.

At this moment the reader may remark that this is the neoliberal theory as well. It is. The Chicago school argues that all transfers of income cause deadweight losses. All support-maximizing governments, it is claimed, are tempered in their zeal for redistribution by the fact that owners of endowments will increasingly withdraw them from productive uses as taxes rise. (Bates and Lien 1985, Becker 1983, and Peltzman 1976 are presentations of formal models.) The difference between the two theories is that neoliberals are "pluralists"; that is, they are agnostic about the groups that have the power of inflicting the losses on the public by withdrawing their endowments. This difference should not obscure, however, the fact that both theories understand in the same way the relation between income distribution and investment.

The belief that under capitalism governments are structurally dependent on capital is widespread. Miliband (1969, 152) portrayed this dependence as follows: "Given the degree of economic power which rests in the 'business community' and the decisive importance of its actions (or its nonactions) for major aspects of economic policy, any government with serious pretensions to radical reform must either seek to appropriate that power or find its room for radical action rigidly circumscribed by the requirements of 'business confidence.' " Block (1977, 15) maintained that "in a capitalist economy the level of economic activity is largely determined by private investment decisions of capitalists. This means that capitalists, in their collective role as investors, have a veto over state policies in that their failure to invest at adequate levels can create major political problems for state managers." Lindblom (1977, 172, 175) observed that "because public functions in the market system rest in the hands of businessmen, it follows that jobs, prices, production, growth, the standard of living, and the economic security of everyone all rest in their hands. . . . In the eyes of government officials, therefore, businessmen do not appear simply as representatives of a special interest. . . . They appear as functionaries performing functions that government officials regard as indispensable." Offe (1975, 234) analyzed the predicament of the state vis-à-vis the private economy: "The political system can only make offers to external, autonomous bodies responsible for decisions: either these offers are not accepted, thus making the attempts at direction in vain, or the offers are so attractive in order to be accepted that the political direction for its part loses its autonomy because it has to internalize the aims of the system to be directed."

The theory of structural dependence can be evaluated in three ways. One

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method is to examine differences in policy choices and outcomes under different governments. Unfortunately, while there is much evidence that some policy outcomes covary with partisan orientations of governments, these findings cast little light on the existence of structural constraints that bind all governments. We cannot know whether the observed differences exhaust the realm of possibility. (The classical study was Hibbs 1977. See Cameron 1984, Castles 1982, Maravall 1979, and Shalev 1983 for literature reviews.) The second method is to examine the limiting cases: the experience of governments that came into office with programs of major transformations, in particular, programs of simultaneous nationalization of some industries and redistribution of income (Kolm 1977). All such governments either compromised their original program, including total reversals, like that committed by the French Socialists, or provoked profound economic crises and were overthrown by force, as was the government of the Unidad Popular in Chile. These experiences prompted Rueschemeyer and Evans (1985, 62) to observe that "even in countries where the state appears to be in the strongest position relative to private capital, . . . the state remains dependent on private capital, foreign and domestic, not only to promote accumulation but also to produce a surplus in which the state itself may share. The strict limits under which the state must operate in a dependent capitalist economy are grimly indicated by the severe problems confronted by social democratic regimes such as those of Salvador Allende in Chile and Michael Manley in Jamaica." Yet again, these experiences cannot speak to the issue of limits and possibilities: one can always cite some counterfactual actions that would have perhaps avoided the disasters. The issue concerns possibility and possibilities cannot be determined on the basis of limited historical experience. Hence, finally, the appropriate method to evaluate the hypothesis of structural dependence is to construct a formal model with which the internal logic of the theory can be explored and the robustness of its conclusions examined.

The Theory Reconstructed

Assumptions

To examine the validity of this theory, we need some assumptions. Suppose the society is composed only of wage earners and owners of capital. Wage earners consume all of their incomes and own no capital. There are firms, which receive the income from capital, invest some part of it, and distribute the rest to the shareholders. Let the net national income at time τ be $Y(\tau)$, let aggregate wages be $W(\tau)$, let investment be $I(\tau)$, and let profits net of investment be $P(\tau)$. Then $W(\tau) + I(\tau) + P(\tau) = Y(\tau)$. Let us implicitly define the share of wages in the national product, m, as $W(\tau) = mY(\tau)$. The volume of investment is $I(\tau) = s(1 - m)Y(\tau)$, where s is the rate of investment out of the total return to capital, $(1 - m)Y$. The volume of profits net of investment is then $P(\tau) = (1 - s)(1 - m)Y(\tau)$.

We make the simplest possible assumption about production: output is a linear function of capital. Expressed in terms of growth, $Y'(\tau) = \nu I(\tau) = \nu s(1 - m)Y(\tau)$, $\nu > 0$, where ν is the constant productivity of capital. The parameter ν represents the state of technology—the amount of additional output that can be produced when an additional unit is added to the capital stock. Thus, output grows at the rate of $\nu s(1 - m)$ per year. For example, if $\nu = .25$, $s = .50$ and $m = .80$, the economy is growing at an annual rate of 2.5%.[1]

Firms make investment decisions. They are perfect agents of their homogeneous shareholders. Thus, firms maximize the utility that their shareholders derive from

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profits.[2] Shareholders derive their utility from present and discounted future consumption, where ϱ is the rate at which the future is discounted. For the sake of algebraic simplicity, we assume shareholders' utility in each period is given by a logarithmic utility function.[3] Let P^* represent the present value of shareholders' welfare: the value they attach today to a particular sequence of present and future consumption. Then P^* can be written

$$P^* = \int_0^\infty e^{-\varrho\tau} \ln[P(\tau)]d\tau, \ \varrho > 0. \qquad (1)$$

Finally, wage earners derive their utility from consumption in the same manner.[4] Wage earners' welfare is given by

$$W^* = \int_0^\infty e^{-\varrho\tau} \ln[W(\tau)]d\tau.$$

These assumptions are perhaps the simplest possible ones. Many can be complicated to introduce more descriptive realism without altering the qualitative conclusions. In the hope of remaining accessible to a wider audience, we assert the analytic results without proof in the text. Complete derivations are contained in the Appendix.

Structural Dependence without the State

Consider the economy without state intervention. The state is there: it enforces contracts, standardizes weights and measures, and the like, but it does not touch income or investment.

Firms choose a rate of investment to maximize P^* in Equation 1 given that the share of wages in income is m. The solution to this problem (for $m < 1$), as shown in the Appendix, is

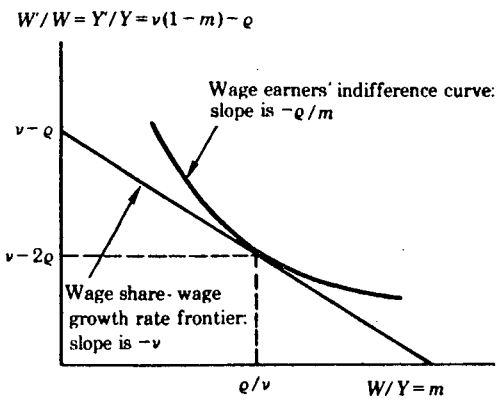
$$s^*(m) = 1 - \frac{\varrho/\nu}{1 - m} = s^M, \qquad (2)$$

where s^M stands for "market rate."[5] This is the investment rate firms will choose in the absence of taxes. Note that the rate of investment falls as the share of wages increases, $ds^*/dm < 0$.[6] Note also that the rate of investment is always less than 1, and it may be less than 0. Firms may find it optimal to disinvest. Indeed, as m approaches 1, s^M approaches negative infinity. Note, finally, that without taxes, shareholders are consuming the income share $(1 - s^*)(1 - m) = \varrho/\nu$, and firms are investing the share $I/Y = 1 - \varrho/\nu - m$ of national income. If $\varrho/\nu > 1$ or if the rate at which shareholders discount the future exceeds the productivity of capital, firms will disinvest, whatever the wage share.

Because shareholders always maintain their current consumption at ϱ/ν of income, any increase in the wage share must be exactly offset by a decline in the share of income going to investment: $d(I/Y)/d(W/Y) = -1$. Wage earners thus face a trade-off between the wage share and the rate of growth of national income (which determines the rate of growth of wages) as illustrated in Figure 1.

The straight line represents all possible combinations of the wage share and the

Figure 1. Wage Earners' Trade-off between Wage Share and Wage Growth



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rate-of-wage growth as income is transferred from wages to investment and vice versa. If wage earners were organized in a centralized union federation with sufficient power to set wages unilaterally, they would maximize W^* by choosing the point on the wage share–wage growth rate frontier that is tangent to their indifference curve where the wage share equals $m^{**} = \varrho/\nu$.[7] Regardless of the manner in which wages are determined, the freedom of firms to determine the rate of investment suffices to maintain shareholders' current consumption share constant. In the absence of government intervention, wage earners can only increase wages at the expense of investment and, hence, of future wage increases. Wage earners are structurally dependent.

Structural Dependence of the State

Now let us consider a government that taxes and transfers income. The government imposes a tax either on income from capital or on wages and transfers the collected revenue to the other group. Taxes, T, are collected according to $T = t(1 - m)Y$, where t is the tax rate. If the tax rate t is positive, the tax is imposed on income from capital, and the revenue is transferred to wage earners. If the tax rate t is negative, the tax is imposed on wages, and the revenue is transferred to firms.[8]

Wage earners' share of national income is now $(W + T)/Y$, while the amount of net profits is

$$P = (1 - m)Y - I - T$$
$$= (1 - m)(1 - s - t)Y.$$

The rate of investment firms choose now, given the tax rate and the share of wages in income, is

$$s^*(m,t) = (1 - t) - \frac{\varrho/\nu}{1 - m}$$
$$= s^M - t.$$

With a tax on income from capital, tax payments come entirely out of investments. With a tax on wages, tax payments go entirely into investments. Shareholders continue to consume the same share of income as before, since $[1 - t - s^*(m,t)] = (1 - s^M)$. The share of income that is invested with taxes is $I/Y = 1 - m - \varrho/\nu - (1 - m)t$.

Thus the trade-off faced by the government between redistribution of income through taxation and investment is identical to the trade-off between wages and investment: $d(I/Y)/d(T/Y) = -1$. We can relabel the horizontal axis of Figure 1 to read $(W + T)/Y = m + t(1 - m)$, and use the same line to describe the possibilities facing the government as it raises or lowers the tax rate. The state is in the same situation as wage earners are.

What will different governments do when confronted with this trade-off? Because shareholders' welfare declines as the tax rate increases, $dP^*/dt < 0$, a pro-business government would tax wage earners and transfer the income to firms: it would generate growth financed by workers. A purely procapitalist government, a government which sought to maximize P^*, would tax wages at the maximum feasible rate. A purely prolabor government, on the other hand, which sought to maximize W^* would have the same indifference curves as wage earners in Figure 1. Such a government would choose the tax rate such that $(W + T)/Y = m + t(1 - m) = \varrho/\nu$, or $t = (\varrho/\nu - m)/(1 - m)$.[9]

Thus procapital governments favor growth over wage earners' consumption while prolabor governments prefer greater consumption for wage earners at the cost of slower growth. No government, however, can reduce the share of income that owners of capital consume. Any additional income for wage earners, whether it consists of wage gains won at the bargaining table or of transfer payments won through elections, reduces

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total investment, dollar for dollar. As long as $\varrho/\nu < 1$, wage earners will moderate their claims on profits and so will governments representing wage earners.

Given this conclusion, it is not surprising that democracy appears so feeble when faced with the power of capital. "The institutional *form* of this state," write Offe and Runge (in Offe 1984, 121), "is determined through the rules of democratic and representative government, while the material *content* of state power is conditioned by the continuous requirements of the accumulation process." "The presumed sovereignty of the democratic citizenry," Bowles and Gintis (1986, 90) state succinctly, "fails in the presence of capital strike." The democratic process may bring governments with radically different bases of support into office but not governments with radically different programs.

Is the State Structurally Dependent on Capital? Statics

Taxes on Consumption out of Income from Capital

These conclusions are based, however, on an assumption that need not be true of capitalism in general. This assumption is that the only policy instrument governments can possibly use is a tax on income. This particular policy instrument leaves the state structurally dependent because utility-maximizing shareholders respond to tax increases with less investment. But governments can adopt policies that alter the trade-off between investment and income distribution. The inference that because society is dependent on capital, so must be the state is false.

In fact, all governments in developed capitalist countries tax, at different rates, the part of income from capital that is invested and the part that is consumed. Tax incentives for investment have been widely used, at least since the end of World War II. Depreciation allowances that differ from the rate at which plants and equipment actually wear out or become obsolete, investment credits, investment grants, special treatment of capital gains, and double taxation of profits distributed as dividends are only some of the most common deviations from a straight income tax. (For recent reviews of investment incentives in different countries, see Bracewell-Milnes 1980; Bracewell-Milnes and Huiskamp 1977; Commission of the European Community 1975; Hogan 1967; King and Fullerton 1984; Kopits 1981; Organization for Economic Cooperation and Development [OECD] 1979, 1983, 1984; Price, Waterhouse & Co. 1981.)

Let us distinguish the two taxes. The tax imposed on all income from capital (gross profits) is $T_i = t_i(I + P) = t_i(1 - m)Y$. We will refer to it as the "income tax." This is the tax we analyzed in the preceding section.

The tax imposed on shareholders' consumption is $T_c = t_cP = t_c(1 - s)(1 - m)Y$. This is the "consumption tax."[10]

Suppose now that a government adopts a pure consumption tax, that is, $t_i = 0$ so that $t_c(1 - s)$ is the tax rate applied to the income from capital $(1 - m)Y$. The most important effect of using this instrument is that the rate of investment is not affected by this tax rate, whatever it is, as long as wages are constant (Auerbach 1983). Indeed, as shown in the Appendix, for any rate of taxation of consumption out of profits, firms continue to invest at the market rate given by Equation 2:

$$s^*(m) = 1 - \frac{\varrho/\nu}{1 - m} = s^M.$$ [11]

We are thus already one step away from structural dependence: with this tax instrument a government can keep investment at its market determined rate and

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distribute the remaining income of shareholders to wage earners. A pure prolabor government can set t_c close to 1, reduce shareholders' share of consumption almost to 0, yet keep the rate of investment unchanged at s^M.

This much is true when the wage share is fixed. But in some capitalist countries wage earners are highly organized in centralized union federations with the capacity to shape the private distribution of income. If a government uses a pure consumption tax, the union federation will maximize W^* by choosing the share of income given by $m^{**}(t_c) = (1 - t_c)\varrho/\nu$. The willingness of the unions to engage in the political exchange described by Pizzorno (1978), Hibbs (1978), and Korpi and Shalev (1980), in which lower wages are traded for increases in welfare programs is apparent: as the tax rate, and thus transfers of income, increase, the federation reduces its preferred private wage share.

Because consumption taxes do not affect the rate of investment and do reduce the share of wages that centralized unions will demand and because the rate of investment increases as the share of wages declines (Equation 2), increases in the tax on consumption out of profit can increase the rate of investment. The rate of investment resulting from a tax on consumption of income from capital when wages are set unilaterally by centralized unions will be

$$s^*[m^{**}(t_c)] = 1 - \frac{\varrho/\nu}{1 - (1 - t_c)\varrho/\nu},$$

which is a positive function of t_c, $ds^*/dt_c > 0$.[12] This, then, is the second step: in concertation with the union federation the government can increase investment.

Can it do both: reduce the income share consumed by shareholders and increase investment above market-determined levels? The answer is positive; but because an even stronger conclusion can be sustained, let us go directly there.

Workers' Welfare under Socialism and Democratic Capitalism

The strong result is a vindication of the traditional social democratic strategy, formulated perhaps for the first time by Bernstein in 1898, elaborated by Henrik de Man in 1933, adopted by the Swedish Social Democrats in the 1930s, and codified in the German Social Democratic Programme of Bad Godesberg in 1959, namely, that a proworker state can control the capitalist economy in a manner that would bring the material welfare of wage earners to the same level they could reach under socialism.

Under capitalism, owners of capital stock control investment, while wage earners may at most control the distribution of private income. The strategic problem organized workers confront under capitalism is thus to choose the share of income, given that someone else determines investment. Under socialism, wage earners would control both the share of income going to investment and that going to consumption. Since capital stock would be collectively owned, there would no profit incomes; and the society as a whole, through some preference-revealing mechanism, would decide the distribution of income into investment and consumption.[13]

Wage earners' strategic problem under socialism would thus be to choose their share of income, knowing that whatever income they do not consume is invested. Technically, this is the same as setting $s = 1$ and choosing m to maximize W^*, which yields $m^*(1) = \varrho/\nu$. The rest of income goes to investment, or $I/Y = 1 - \varrho/\nu$. Associated with this allocation of resources is the level of wage earners' welfare under socialism $W^*(S)$. Given the technology and the time preferences, $W^*(S)$ represents the highest possible level

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of wage earners' welfare—the level they could attain if they controlled investment as well as income distribution.

Back to capitalism: consider the case with one big union and a government that chooses a pure consumption tax with the tax rate t_c close to 1. As t_c approaches 1,

$$(W + T)/Y = [m^{**} + t_c(1 - s^*)(1 - m^{**})] \rightarrow \varrho/\nu$$

and

$$I/Y = s^*(1 - m^{**}) \rightarrow 1 - \varrho/\nu,$$

which is the same as under socialism. As t_c approaches 1, wage earners' welfare under capitalism approaches the welfare they could obtain under socialism (Wallerstein and Przeworski, n.d.).

Would a government ever choose such a tax rate? To answer the question we need a language to characterize governments. Suppose the government's objective is to maximize G^*, which depends on the welfare of wage earners and shareholders according to $G^* = (W^*)^\gamma(P^*)^{1-\gamma}$, $0 < \gamma < 1$. Thus γ is the weight attached to the welfare of wage earners and $(1 - \gamma)$ is the weight given the welfare of shareholders. If γ approaches 1, labor's interests predominate; if γ approaches 0, the government exclusively favors capital.

The government's optimal tax on consumption out of income from capital is given implicitly by the equation

$$t_c = \frac{\gamma P^*}{\gamma P^* + (1 - \gamma)W^*}.^{14} \qquad (3)$$

Thus, as long as $0 < \gamma < 1$, the government will choose to tax consumption out of income from capital at a rate also between 0 and 1. In addition, the greater the weight given to wage earners' welfare, the higher will be t_c, or $dt_c/d\gamma > 0$. Finally, as γ approaches 1, t_c also approaches 1 and wage earners' welfare approaches that associated with socialism. Note that the share of national income that shareholders consume approaches 0, but shareholders' consumption in absolute terms may remain substantial. The conclusion is that when all wage earners are organized in one centralized union federation and the government is purely prolabor it will choose a tax on capitalist consumption the effect of which will be to bring wage earners' material welfare almost to the level they could obtain under socialism. Hence, at least when the union federation is willing to engage in a political exchange of private and public income, the state is not structurally dependent on capital.[15]

Caveats and Preliminary Conclusions

These conclusions are subject to a number of qualifications: (1) the results may depend on restrictive assumptions about workers' and shareholders' preferences; (2) structural dependence may work through mechanisms other than investment; (3) the state may be dependent not on national but foreign investors; (4) interventionist governments may be inherently incapable of serving as perfect agents of any group's interests. Let us consider each in turn.

For the sake of mathematical tractability, we have worked with particularly simple specifications of the production and utility functions. Our conclusion does not depend on the assumption of a linear production function. Auerbach (1983) and Przeworski and Wallerstein (1985) present models of investment-neutral taxes on profits using the standard production function with diminishing returns to investment. Nor is our conclusion restricted to the case of logarithmic utility functions. The same results can be derived with any utility function with a constant elasticity of marginal utility.[16] We do not know, however, the generality of our results within the entire set of quasi-concave utility functions with which economists like to work.

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Our results are sensitive to the assumption that firms care only about the present and future consumption of their shareholders. Owners of firms may not like encroachments on their prerogatives or the paying of taxes per se. Yet, the fact that owners of firms do not like something the government is doing, even if it touches them as profoundly as, say, a law increasing workers' rights at the workplace, is not sufficient to affect investment. Suppose firms maximize some more general utility function $V(P^*,x)$ where P^* is, as above, the welfare derived from present and future consumption and x is anything else that causes distress ($\partial V/\partial x < 0$) as the Left comes to power. "Investment strikes" will occur only if $dx/ds > 0$. Note that control over the workplace will not do for x, because it is independent of investment. Dislike of paying taxes will cause firms to increase, not decrease, their rate of investment in the presence of taxes on shareholders' consumption. Thus, likely candidates for x are not obvious.

Our results are also sensitive to the assumption that workers care only about their consumption. If workers determine the supply of labor by balancing their marginal utility of leisure with their marginal utility of income, the replacement of private wages with transfer payments financed by a tax on consumption out of profits will reduce incentives to work as it increases incentives to invest.

The second objection is that our conclusion is valid at most with regard to a specific mechanism of structural dependence, namely, the dependence of future material welfare on investment. One might postulate other mechanisms that would render the state dependent on capital, for example, the need to finance state activities through borrowing (see Marx 1952b, written in 1850). The mechanism most often suggested by Marxist theoreticians is the following: governments depend on current levels of economic activity, therefore they depend on investment. This formulation is, however, incoherent: if governments are concerned with current levels of activity, current demand, or current employment, they can and would do many things other than worry about investment. In a Keynesian textbook they can reduce taxes on wages, increase public employment, or print money as well as reduce interest rates to stimulate investment. A concern with the short run would not lead to a preoccupation with investment. Investment matters mostly for the long run.

In general, however, it may be true that the state is structurally dependent through mechanisms other than the one analyzed here. A reasonable short-term model of dependence is yet to be developed, but our conclusion should not be taken to be more general than it is.

Third is the issue of international constraints. A clarification may be needed. In the analysis, export of capital was treated as equivalent to consumption from the point of view of the tax system: whatever is not invested in the particular economy is subject to the tax. Thus all that governments need to do, in order to use the tax system to induce domestic (as distinct from foreign) investment, is to ascertain the actual amount of domestic income and investment. And in spite of transfer pricing and other mechanisms by which firms can shift tax burdens from country to country, there seems to be a general agreement that by and large, governments do monitor international flows of income (Bracewell-Milnes 1980; Hartman 1985; OECD 1983). Governments that seek to attract new foreign investment are in a different and more constrained situation. High taxes on consumption out of profits that have no effect on the investment decisions of domestic firms (or of local subsidiaries of multinational firms) will deter new foreign investors. The most important and unresolved issue, however, concerns the range of choice that the government of any country has when domestic

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producers must trade in international markets in which competitiveness depends to a significant degree on relative labor costs.

Finally, we come to the problem of the limits of governmental competence. Government intervention in the economy creates a potential for vast inefficiencies in the allocation of investment and labor. Any system of investment incentives is vulnerable to inefficiencies due to differing effective rates on different assets (King and Fullerton 1984). Any system in which private consumption depends primarily on transfers rather than wages is vulnerable to inefficiencies due to misallocation of labor. The problem lies not only in the difficulty of designing neutral taxes or transfers but in their implementation. The very fact that the state acquires the capacity to intervene in the economy, that is, to affect material welfare of private actors, turns it into an attractive target of influence by these actors. What has thus developed as the eventual aftermath of the Keynesian revolution is a state that is strong in its capacity to intervene and that is highly vulnerable to private influence (Schmitter 1985, Skidelsky 1977). Diagnoses formulated within ideologically distant perspectives coincide in their antistatist conclusions: Habermas's (1975) analysis of the crisis facing capitalism is identical to that of Stigler (1975). What may be thus wrong with our analysis is the assumption that interventionist governments can ever serve as perfect agents of the political forces that placed them in office. The economy is sick, the drugs are available, but the doctor may be a hack.

In sum, there may be limits to the ability of the state to redistribute consumption without discouraging private investment, because the economy depends on an inflow of capital from abroad (rather than the reinvestment of profit earned at home) or because private wages cannot be replaced by public income without undermining work incentives or because the government cannot be trusted with the discretion required by redistributive policies. But all of these reasons are less general than the claims of the theory of structural dependence.

Our analysis reveals a large range of economically feasible policies. Exclusive reliance on income taxes, that is, taxes that are independent of the rate of investment—whether these are imposed on capital or labor—allows owners of capital to choose the share of national income they want to consume and to ward off, by reducing the share of investment, all attempts both by unions and by governments to redistribute consumption. Under exclusive reliance on income taxes, the consumption of all groups other than shareholders, government included, is thus subjected to "market discipline." In contrast, if only consumption out of income from capital is taxed, governments can redistribute consumption without reducing investment. Acting in concert with unions willing and able to trade off wage demands for increased transfer payments, the government can increase investment and reduce capitalists' consumption share simultaneously.

Governments can choose different policies concerning distribution and growth and these policies can be effective. Specifically, governments can increase investment by reducing wage earners' consumption or by reducing the consumption of owners of capital. Since all social groups will seek to shift the costs of investment onto others, the outcome of the democratic process can matter greatly for the welfare of different groups under capitalism. The state is not structurally dependent.

These conclusions may seem banal, but they run against a substantial body of established theory. The Marxist theory presented in the first section is logically persuasive and historically plausible. Moreover, we demonstrated in the second section that as long as governments limit

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themselves, or are limited, to using a flat income tax, the state is structurally dependent on capital. And, importantly, Marxists are not the only theoreticians who come to the conclusion that the state is dependent: the language is different but this is the general thrust of theories of the state originating from the neoclassical economic theory, as exemplified by Peltzman (1976), Aumann and Kurz (1977), Busch and Mackay (1977), Becker (1983), Bates and Lien (1985), and innumerable others who conclude that all governments seeking support must anticipate the negative effect of taxes on private investment. Thus, there are good reasons to be theoretically surprised.

Is the State Structurally Dependent on Capital? Dynamics

Once they are in office, prolabor governments can use tax policies to bring the welfare of wage earners arbitrarily close to the level wage earners could attain if they controlled investment directly: this is the central conclusion we have reached thus far. This conclusion, however, holds only once the prolabor government is in office—more precisely, once the policy is in effect. The question that remains is what happens to investment and wage earners' welfare when firms anticipate that a prolabor party would or might win the next election and institute such tax policies.

Since the mathematical analysis is quite complicated, we present only the framework of analysis and the conclusions. Suppose that at time $\tau = 0$ a government characterized by γ_0 is in office and the tax rate is $t_c = t_0$. The government is highly procapitalist and this tax rate is close to 0. An election is to take place and firms expect that this election will be won by a prolabor government characterized by $\gamma_1 > \gamma_0$, which will eventually introduce, at time $\tau = 1$, a tax $t_c = t_1 > t_0$.[17] What rate of investment will be chosen during the period from $0 \leqslant \tau < 1$ if firms expect that tax rate to be higher at and after $\tau = 1$.

When firms do not anticipate a change in tax policy, they choose the rate of investment that maximizes $P^*(t_0)$ between $\tau = 0$ and infinity. If, however, they anticipate that the tax will increase at $\tau = 1$ and remain at the new level, they will choose a time path of investment $s(\tau)$ to maximize

$$\int_0^1 e^{-\varrho\tau}\ln[P(t_0, \tau)]d\tau + \int_1^\infty e^{-\varrho\tau}\ln[P(t_1, \tau)]d\tau.$$

The conclusion is the following: firms that anticipate a future increase in taxes will reduce investment in the period $0 \leqslant \tau < 1$ even if the tax is a pure consumption tax that will not lower investment in the period $\tau \geqslant 1$. The same holds for firms that are not certain about future tax policy but think taxes will rise with some positive probability. Thus, anticipations of certain or possible increases of taxes lead firms to lower their investment rate. Similarly, expectations of future tax decreases will cause current investment to increase. Pure consumption taxes are no longer investment-neutral when change is anticipated.

Thus "business confidence"—the expectation by firms concerning government policy—has direct consequences for the investment behavior of firms. Yet this conclusion should be distinguished from the view of Kalecki (1966; also Block 1977), who also saw in business confidence the constraint that any government under capitalism is forced to consider. In Kalecki's view, business confidence threatened investment even in a static case, when the government policy was already in place and no changes were anticipated. However, once a government

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has implemented a consumption tax, the investment rate is unaffected by the tax rate. It is the anticipation that a government would introduce or raise such a tax that erodes business confidence and lowers investment.[18]

We suspect that many newly elected proworker governments abandoned or moderated their programs for wrong reasons, having mistaken the cost of anticipation of policies for the costs of policies themselves. The French Socialist party announced in the electoral campaign of 1981 that it would propose a law in favor of renters. Once elected, it prepared such a bill. In the meantime, the vacancy rate went up and so did rents. Having listened to a chorus of right-wing press, the government decided that the policy was too expensive and moderated the provisions of the law. Our analysis suggests that this may have been a mistake. The government never waited to discover the costs of the law. Instead, it panicked over the costs of anticipation which, by then, were sunk.

Ever since Winston Churchill sneered in 1924 that the Labour party was not fit to govern, parties of labor have been making desperate efforts to show their moderation and responsibility in administering the economy once in office. Yet once the decision to adopt a policy has been made and once the government has suffered the effects of anticipation, the government is best-off introducing the original program without any compromises. The time to compromise is not after the costs have already been paid.[19]

Since anticipated transitions to higher taxes are costly for wage earners' welfare, socialists are sometimes tempted to proceed "in one stroke"—to use the phrase of Oskar Lange, who thought that if a government has decided to go ahead with a socialist program at all, "it must go through resolutely . . . at a maximum speed" (1964, 125; also Kautsky 1925). This is not a new idea: Machiavelli advised, "When a prince takes a new state, he should calculate the sum of all the injuries he will have to do, and do them all at once, so as not to have to do new ones every day; simply by not repeating them, he will thus be able to reassure people, and win them over to his side with benefits" (1977, 28).

Our analysis suggests that socialist governments should never announce more than they intend to do and that they should do what they announced fully and quickly. Leon Blum was wrong to have waited to assume office after the Popular Front won the elections of 1936. Yet we do not know whether one stroke is better than many. Indeed, we cannot even determine whether or not workers' parties should seek to increase the tax on shareholders' consumption. We know that once instituted, an increase in t_c will increase wage earners' welfare until or unless the socialist government is defeated and the policy reversed. But we also know that anticipation of this moment causes a fall in investment and is costly to wage earners' welfare in the period before the tax increase goes into effect. Thus, the question remains open whether the welfare of wage earners is improved. If workers are better-off when prolabor governments adhere to the status quo, then the state is structurally dependent in dynamic terms: some distributions of consumption are unattainable because the transition costs are too high.

Structural Dependence and Socialist Policies

The capitalist economy, in which owners of wealth and of the capacity to work make decentralized decisions about allocations of their endowments, regularly generates a number of effects that are experienced as profound deprivation by large segments of society. The market does not assure the material security of

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anyone who does not own wealth and is not able to earn a living. The market generates drastic inequalities, including inequality of opportunity. Finally, in cases where the private rate of return diverges from the social one, the market allocates resources to uses inefficiently. This has been the traditional socialist view of capitalism.

The perennial question has been what, if anything, can be done about it. Social Democrats in several countries developed a full-fledged program for steering the capitalist economy in the interest of people who depend on income from employment or cannot earn an income sufficient to survive. By controlling the capitalist economy, the state run by socialists could rationalize the economy as a whole and orient it toward the general welfare, the Swedish Social Democratic minister of finance discovered in the 1930s (Wigforss, as summarized by Lewin 1975, 286). Because capitalism became organized, it could be administered by socialists in the interest of the people, argued the eventual German Social Democratic minister of finance (Hilferding 1981). Ironically, it was Keynes who provided the policy instruments for controlling capitalist economies and perhaps the most eloquent statement of the social-democratic position: "It is not the ownership of the instruments of production which it is important for the state to assume. If the state is able to determine the aggregate amount of resources devoted to augmenting the instruments and the basic reward to those who own them, it will have accomplished all that is necessary" (1964, 378).

Even in its heyday, however, the social-democratic strategy was the subject of at least skepticism if not hostility from those who were persuaded that no government, not even one elected by "the people," could control investment and income distribution as long as the instruments of production were privately owned. The thesis that the state is structurally dependent on capital was used by critics of social democracy to argue that this strategy is unfeasible, since the ability of any government to determine the level of investment and the distribution of income is subject to a trade-off given by the self-interest of owners of the instruments of production.

Our contribution to this debate is as follows: Once in place, a tax on shareholders' consumption does not reduce investment. The state is not structurally dependent on capital in the sense that virtually any distribution of consumption between wage earners and shareholders is compatible with continual private investment. Yet anticipations of future threats to shareholders' consumption do reduce investment and impose costs on wage earners. Whether and when the future benefits to wage earners outweigh the costs incurred during the transition period remains an open question.

Appendix

Preliminaries

Given that the economy grows according to $Y'(\tau) = \nu s(1 - m)Y(\tau)$, the national income at any time is given by $Y(\tau) = Y(0)e^{\nu s(1-m)\tau}$. Wages, profits, investment, and taxes all constitute shares of this income as described in the text. All of our results are derived from the first-order conditions of a series of maximization problems. In all cases, it is easily verified that the second-order conditions for a maximum are satisfied.

Capitalists' Problem

Firms choose s to maximize

$$P^* = \int_0^\infty e^{-\varrho\tau}\ln[P(\tau)]d\tau = \frac{1}{\varrho}\left[\ln P(0) + \frac{\nu s(1-m)}{\varrho}\right]. \quad \text{(A-1)}$$

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Note that P^* is only well defined when $P(0) > 0$. To insure that this condition is met, we assume throughout that $m < 1$ or $\varrho/\nu < 1$.

Without taxes, $P(0) = (1 - s)(1 - m)Y(0)$. Firms thus choose the s that satisfies

$$\frac{\partial P^*(s,m)}{\partial s} = \frac{1}{\varrho}\left[\frac{\nu(1-m)}{\varrho} - \frac{1}{1-s}\right] = 0$$

which implies the choice

$$s^*(m) = 1 - \frac{\varrho/\nu}{(1-m)} = s^M. \qquad \text{(A-2)}$$

Capitalists are consuming the share $(1 - s^M)(1 - m) = \varrho/\nu$ and investing the share $s^M(1 - m) = (1 - m - \varrho/\nu)$.

With income and consumption taxes,

$$P(0) = [(1 - t_c)(1 - s) - t_i](1 - m)Y(0).$$

The first-order condition is now

$$\frac{\partial P^*}{\partial s} = \frac{1}{\varrho}\left[\frac{\nu(1-m)}{\varrho} - \frac{1-t_c}{(1-t_c)(1-s) - t_i}\right] = 0,$$

which yields

$$s^*(m,t_i,t_c) = 1 - \frac{\varrho/\nu}{(1-m)} - \frac{t_i}{(1-t_c)}. \qquad \text{(A-3)}$$

In the case with only income taxes, $t_c = 0$ and Equation A-3 reduces to

$$s^*(m,t_i) = 1 - \frac{\varrho/\nu}{(1-m)} - t_i = s^M - t_i. \qquad \text{(A-4)}$$

Capitalists are consuming the share $(1 - s^* - t_i)(1 - m) = \varrho/\nu$ and investing the share $s^*(1 - m) = [(1 - m)(1 - t_i) - \varrho/\nu]$.

In the case with only consumption taxes, $t_i = 0$ and Equation A-3 reduces to

$$s^*(m,t_c) = 1 - \frac{\varrho/\nu}{(1-m)} = s^M. \qquad \text{(A-5)}$$

Now capitalists are consuming the share $(1 - t_c)(1 - s^*)(1 - m) = (1 - t_c)\varrho/\nu$ and investing the share $s^*(1 - m) = (1 - m - \varrho/\nu)$. As t_c goes to 1, capitalists' consumption share goes to 0.

Workers' Problem

Unions choose m to maximize

$$W^* = \int_0^\infty e^{-\varrho\tau}\ln[W(\tau) + T(\tau)]d\tau = \frac{1}{\varrho}\left[\ln[W(0) + T(0)] + \frac{\nu s(1-m)}{\varrho}\right]. \qquad \text{(A-6)}$$

If there are no taxes and the unions treat s as exogenous, $W(0) = mY(0)$ and the solution to Equation A-6 (for $s > 0$) is given by

$$\frac{\partial W^*(m,s)}{\partial m} = (1/\varrho)[(1/m) - (\nu s/\varrho)] = 0,$$

which implies $m^*(s) = \varrho/\nu s$.

If firms invest according to their best reply $s^*(m)$ given by Equation A-2, $\nu s^*(1 - m)/\varrho = [\nu(1 - m)/\varrho - 1]$ in Equation A-6 and the solution to the workers' problem becomes

$$\frac{dW^*[m,s^*(m)]}{dm} = (1/\varrho)[(1/m) - (\nu/\varrho)] = 0$$

with $m^{**} = \varrho/\nu$.

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With income and consumption taxes, $[W(0) + T(0)] = \{m + [t_i + t_c(1 - s)](1 - m)\}Y(0)$. If unions anticipate capitalists' response $s^*(m,t_i,t_c)$ as given by Equation A-3, the workers' solution is given by

$$\frac{dW^*}{dm} = \frac{1}{\varrho}\left[\frac{1 - A}{m + (1 - m)A + t_c\varrho/\nu} - \frac{1 - A}{\varrho/\nu}\right] = 0 \qquad \text{(A-7)}$$

where $A = t_i/(1 - t_c)$. Solving for m^{**},

$$m^{**}(t_i, t_c) = \frac{(1 - t_c)^2\varrho/\nu - t_i}{1 - t_c - t_i}. \qquad \text{(A-8)}$$

Note that when $s = s^*(m,t_c,t_i)$, workers' share $(W + T)/Y = [m + (1 - m)t_i/(1 - t_c) + t_c\varrho/\nu]$. Therefore it follows immediately from Equation A-7 that when workers anticipate capitalists' best response s^* in choosing m, workers consume the share ϱ/ν of national income.

In the case with only income taxes, $t_c = 0$ and Equation A-8 reduces to $m^{**}(t_i) = (\varrho/\nu - t_i)/(1 - t_i)$. In the case with only consumption taxes, $t_i = 0$ and Equation A-8 reduces to

$$m^{**}(t_c) = (1 - t_c)\varrho/\nu. \qquad \text{(A-9)}$$

The Government's Problem

The government's objective function depends on the welfare of workers and capitalists according to the equation $G^* = (W^*)^\gamma(P^*)^{1-\gamma}$, $0 < \gamma < 1$. To find the government's optimal tax, we take the derivative

$$\frac{dG^*}{dt_j} = (W^*)^{\gamma-1}(P^*)^{-\gamma}\left[\gamma(P^*)\frac{dW^*}{dt_j} + (1 - \gamma)(W^*)\frac{dP^*}{dt_j}\right] = 0 \qquad \text{(A-10)}$$

where t_j represents either t_i or t_c. Two cases were discussed in the text.

First we consider the income tax that would be chosen by a purely proworker government. Note that if $\gamma = 1$, then $G^* = W^*$. Thus, we substitute $s^*(m,t_i)$ (Equation A-4) into W^* (Equation A-6) and differentiate to obtain

$$\frac{dW^*}{dt_i} = \frac{(1 - m)}{\varrho}\left[\frac{1}{t_i(1 - m) + m} - \frac{\nu}{\varrho}\right] = 0,$$

which can be simplified to yield the solution $t_i = (\varrho/\nu - m)/(1 - m)$.

Now consider the case with a pure consumption tax, $t_i = 0$, and with unions choosing $m = m^{**}$. Substitute $m^{**}(t_c)$ (Equation A-9) and $s^*(m^{**},t_c)$ (Equation A-5) into W^* (Equation A-6) and take the derivative

$$dW^*/dt_c = 1/\varrho \qquad \text{(A-11)}$$

Similarly, substitute $m^{**}(t_c)$ and $s^*(m^{**},t_c)$ into P^* (Equation A-1) and differentiate

$$dP^*/dt_c = -t_c/\varrho(1 - t_c). \qquad \text{(A-12)}$$

Combining Equations A-11 and A-12 with A-10 and rearranging terms yields $\gamma(1 - t_c)P^* - (1 - \gamma)t_cW^* = 0$, or

$$t_c = \frac{\gamma P^*}{\gamma P^* + (1 - \gamma)W^*}, \qquad \text{(A-13)}$$

which implicitly defines the optimal t_c (since both P^* and W^* contain t_c). Thus, $0 < t_c < 1$ (since $0 < \gamma < 1$). It is readily checked that W^* does not explode as γ approaches 1. Therefore, as γ approaches 1, $(1 - \gamma)W^*$ approaches 0 and t_c approaches 1. Similarly, it is easily checked that P^* does not explode as γ approaches 0. Thus as γ goes to 0, γP^* goes to 0 and so does t_c. To find $dt_c/d\gamma$, differentiate Equation A-13 implicitly and rearrange terms to write

$$\frac{dt_c}{d\gamma} = \frac{(1 - t_c)p^* + t_cW^*}{\gamma P^* + (1 - \gamma)W^* + t_c/\varrho} > 0.$$

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Notes

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1. These figures are rough estimates for the postwar U.S. economy. Estimates of the productivity of capital can be found in Hill 1979. The others can be found in the National Income and Product Accounts published by the Department of Commerce. Of course, investment in physical capital is only one source of economic growth. We emphasize the role of investment in growth because that is the way the theory of structural dependence has been formulated.

2. Alternatively and equivalently, firms maximize gross profits and distribute all income accruing to capital to shareholders, who then choose the rate of savings that maximizes their utility (see Hirshleifer 1970).

3. We use the log function to simplify the presentation, but all of our results hold for the entire class of utility functions with a constant elasticity of marginal utility.

4. Here we consider the rate of time preference ϱ to be risk-free and thus the same for all individuals who derive utility from consumption, whether they are workers or capitalists. We took a different approach in our earlier analysis of class conflict (Przeworski and Wallerstein 1982a), where rates of time preference reflected the differential risk faced by owners of firms and wage earners. Given the problem at hand, it seems better to consider time preferences risk-free and to model uncertainty explicitly. The effects of uncertainty are discussed in the final section.

5. When workers' share m is greater than or equal to unity, profits are negative and firms will disinvest at the maximal rate.

6. We treat investment in neoclassical fashion as the result of an intertemporal allocation of consumption by owners of capital. The Keynesian approach, in contrast, emphasizes the importance of investing to satisfy future demand. In a Keynesian model, the derivative ds^*/dm would be positive since wage increases put more money in the hands of people with a high propensity to consume. The problem of structural dependence cannot arise within a Keynesian framework where increasing wage earners' consumption accelerates investment and may even increase current profits. Indeed, one of the political appeals of Keynesian theory was the denial of any conflict of interests between wage earners and capitalists (see Skidelsky 1977 and Przeworski and Wallerstein 1982b).

7. In technical language, workers are behaving as Stackelberg leaders, firms as Stackelberg followers. This means that the unions anticipate the effect of their choice of wage share on firms' investment decisions, while firms respond optimally to whatever unions choose. We do not, in this paper, consider the alternative of firms leading and unions following, although we did previously in Przeworski and Wallerstein 1982a. The role of Stackelberg leaders poses a collective-action problem for firms. To act as Stackelberg leaders would demand that firms collectively invest more than is optimal for each individually.

We use the double asterisk to denote the optimal choice of the Stackelberg leader and the single asterisk to denote the best reply strategy or the optimal choice of the Stackelberg follower.

8. In this case, the rate at which wages are taxed is given by the equality $T = -t(1 - m)Y = t_w mY$, which implies that the tax rate imposed on wages is $t_w = -t(1 - m)/m$.

9. Wage earners who acquire complete control over tax policy through electoral victories achieve no more and no less than wage earners who gain complete control over private income distribution through collective bargaining: the outcome is the same in both cases. Note that the wage earners' optimal tax may be negative.

10. The total tax rate imposed on income from capital is thus $t = t_i + t_c(1 - s) = (t_i + t_c) - t_c s = t_0 - t_1 s$, where $t_0 = t_i + t_c$ is the nominal tax rate and $t_1 = t_c$ is the rate of investment relief.

11. This result may be nonintuitive: firms pay higher taxes and invest at the same rate as before. The reason is that this tax imposes the same cost on consumption now and in the future and thus does not change the optimal allocation of consumption over time.

12. This follows as long as the government chooses the tax rate t_c such that $(1 - t_c)\varrho/\nu < 1$. Otherwise firms will disinvest at the maximal rate no matter what the government does.

13. Obviously this notion of socialism should not be confused with Eastern European economies, where the decisions about allocation of resources are made by party bureaucrats.

14. Both P^* and W^* contain t_c so, contrary to appearances, t_c is not isolated on the left-hand side of Equation 3.

15. Whether a prolabor government in countries where unions are fragmented or weak could still bring wage earners' welfare arbitrarily close to the level they could attain under socialism with an appropriate set of taxes and transfers remains to be investigated.

16. The family of utility functions with a constant elasticity of marginal utility is conventionally given by the formula $U(c) = [1/(1 - b)]c^{1-b} + B$, $b > 0$, for $b \neq 1$ and $U(c) = \ln(c)$ for $b = 1$. The parameter b is the elasticity of marginal utility.

17. It makes no difference in the analysis if firms are certain taxes will increase at $\tau = 1$ or attribute

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some positive probability to a tax increase at $\tau = 1$. Uncertainty can be understood in this context as a situation in which firms attach subjective probability distributions to the outcomes of future elections or directly to future tax rates. Exactly the same analysis would apply if firms are uncertain about the future wage share or the future productivity of capital.

18. Aubin and Goyeau (1986) have found that in France the prospect of an electoral victory of the Left (measured as the product of the support for the Left in the polls and the proportion of the electoral term that has elapsed) contributes significantly to the explanation of investment in a model that includes measures of aggregate demand and the cost of capital.

19. This does not mean, of course, that governments should stubbornly stick with all policies regardless of their effects. Some policies are simply bad policies.

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Adam Przeworski is Professor of Political Science, University of Chicago, Chicago, IL 60615.

Michael Wallerstein is Assistant Professor of Political Science, University of California, Los Angeles, CA 90024.

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MAX WEBER'S LIBERALISM FOR A NIETZSCHEAN WORLD

MARK WARREN
Northwestern University

Weber's commentators often accuse him of lacking a coherent political philosophy because his pluralist-elite theory of democracy seems indifferent to liberal-democratic values. I argue, however, that the core of Weber's political philosophy is a politicized neo-Kantian liberalism, one that produces an ethically significant and positive concept of politics. The problem is rather that Weber's pessimism about institutionalizing positive politics in bureaucratized societies left the ethical core of his political philosophy inexplicit. This introduced a conflict into his thought between his ethical commitments and his assessments of political possibilities. The conflict is compelling because it reflects the contemporary gap between the promise and performance of liberal democracies. At the same time, formulating Weber's problems in these terms helps identify democratic solutions that remain obscure in his assessment of conflicts between bureaucratization and democracy.

Most students of politics know Max Weber as a political sociologist and social theorist, but few think of him as a political philosopher. After all, he left no treatises on ideal forms of government, nor systematic treatments of the place of politics in the human condition, nor extended considerations of normative principles. Nonetheless, there is a coherent set of philosophical problems and commitments that guides Weber's social science and political thought. I argue that Weber's thought includes a commitment to a distinctive conception of politics, one that is, in effect, a politicized neo-Kantian liberalism: he sees politics as a uniquely human activity, one with the potential both to create and to manifest the responsibility and dignity of individuals in an increasingly secularized world. This positive concept of politics is what accounts for Weber's fear that bureaucratization reinforces what I call a *Nietzschean* world—a world in which individuals are unable to create durably meaningful lives through goal-directed actions.

I also argue, however, that Weber failed to carry his Nietzschean problem and positive concept of politics into his model of a possible politics, as expressed in his pluralist-elite theory of democracy. Although Weber's model dramatically illustrated what he saw as an absolute conflict between bureaucratization and democracy, it also failed to express the inner coherence of his political philosophy. I am not, however, interested in Weber's inconsistencies for their own sake. If it turns out that Weber's political philosophy is coherent, and his analylsis of contemporary institutions correct, the discontinuities in his thinking are symptoms of real challenges for democratic theory. The conflicts in Weber's thinking have not only obscured his ethical contributions to democratic theory, but also his real insights into the limits of liberalism and the difficulties of democracy in bureaucratically organized societies. Yet I

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also argue that these conflicts would have been less had Weber elaborated his liberal commitments in substantially democratic directions rather than the elitist direction he in fact chose.

In contrast to extensive commentary on Weber's philosophy of social science, the philosophical commitments of his social and political thought have not benefited from similar attention. There are a few notable and important exceptions, each of which I rely on here. Karl Löwith (1982) has developed Weber's general concern with individual meaning and responsibility in rationalized societies (cf. Merleau-Ponty 1974, chap. 1). Drawing on Weber's neo-Kantian context, Wolfgang Schluchter (1979, 1981) shows how Weber's social science presupposes an underlying concern with historical conditions that produce or undermine capacities for rational action (cf. Brubaker 1984). And Jürgen Habermas (1984) has raised the level of debate by reconstructing the broad conception of rationality that underwrites Weber's sociology of cultural development. What is missing from the literature, however, is an account of how these dimensions of Weber's thought produce a *political philosophy*; that is, criteria that would permit him to judge the relative merits of political regimes. Nor does the literature provide an account of how Weber's political philosophy relates to his *political theory*, that is, to his views on what kinds of political institutions are possible, given the nature of the contemporary world.

Pluralist Liberal-Democracy and the Pluralism of Values

What we do find in the literature are attempts by commentators interested in Weber's political thought to extrapolate a political philosophy from his concern for Germany's political future, since it is here one finds his most committed political arguments. Most strikingly in "Parliament and Government in a Reconstructed Germany" (1978a, app. 2) Weber argues for what we would today call a pluralist-elite model of democracy,[1] both as an appropriate model for Germany and as the best possible political system in mass, industrial societies (cf. Beetham, 1985, 2). Much like Schumpeter later, Weber redefines democracy to fit the facts of mass passivity and high levels of ignorance. He does so by looking at democracy as a mechanism for selecting leaders by institutionalizing competition between elites. From an ethical perspective, liberal-democracy turns out to be a neutral process that leaves out classical liberal-democratic values, values having to do with popular sovereignty and the good of participation. Weber's model is especially distinctive for the importance he placed on strong political leadership with a plebiscitary basis as a means for checking the power of, and providing goals for, increasingly expansive and powerful state bureaucracies (Beetham 1985; Löwenstein 1966, 26–27; Mommsen 1974).

The problem with extracting Weber's political philosophy from this model stems from comparing it to another prominent aspect of his thinking. Commentators are often taken with Weber's account of the impact of secularization on society and politics (Löwith 1982; Merleau-Ponty 1974, chap. 1; Portis 1983). In Weber's view, the breakdown of traditional sources of moral authority meant that there was a greater diversity of values in Western societies. For this reason, he thought that politics would inevitably play a greater role in mediating value conflicts than it had in the past and that political life would be increasingly marked by conflicts of subjective value commitments against backgrounds of coercion.

No doubt because of their prominence in his writings, most commentators take it for granted that Weber's views on the increasing pluralism of values dovetails

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with his pluralistic reformulation of liberal-democratic theory. David Beetham, one of the best commentators on Weber's political thought, writes that in

> an age of competing, and finally irreconcilable, value positions, all philosophical basis to political order in a rationally defensible agreed morality had to be abandoned. Its place was taken by a purely formal or procedural justification for liberal order, in its ability to allow expression to the plurality of competing values within society at large. Weber's philosophical and sociological perspectives thus reinforce one another in a pluralist restatement of the liberal tradition. (1985, 4–5)

Other commentators make similar points about the moral function of leadership in Weber's liberalism (Eden 1984; Mommsen 1974; Strauss 1950, chap. 2).

Weber rejected natural rights and utilitarian foundations for liberalism, the former as insupportable metaphysics and the latter as incompatible with his neo-Kantian image of human dignity—a point to which I shall return. Since Weber never explicitly developed the neo-Kantian bases of his liberalism, his rejection of more traditional approaches has left the impression that his political thought consists primarily in his arguments for parliamentary government and strong leadership, combined with an ad hoc preference for individual freedom and initiative. For this reason, Weber's theory of leadership seems to emerge as a replacement for previously accepted natural-right and utilitarian grounds for liberalism. His liberalism thus seems to leave moral questions to be solved by political conflict. This is why commentators can argue that Weber in effect replaces the moral foundations of liberalism with the value decisions of leaders within the context of competitive parliamentary machinery. For conservative commentators like Leo Strauss (1950, chap. 2), Weber reflects the intrinsically nihilistic logic of liberalism. For liberals such as Wolfgang Mommsen (1974, 93), Weber's emphasis on leadership is a dangerous departure from traditional liberal-democratic ethics and a testimony to his overriding commitment to German nationalism. For social democrats such as David Beetham (1985, 2–7), Weber represents the hollowness of democratic commitments within pluralist-elite theory. These commentators imply that when one chooses to view Weber's liberalism as political *philosophy*—that is, as a defensible set of ethical commitments regarding a desirable political order—rather than simply as a formal theory of leadership in the context of institutional checks and balances, one finds it deeply flawed.

These approaches miss the inner consistency of Weber's political philosophy. For this reason, they also miss crucial and informative ambivalences within his political thought as a whole, especially as they relate to our own political situation today. The problem is that commentators assume that to the extent Weber has a political philosophy, it is fully reflected in his pluralist-elite theory of democracy. The assumption is wrong: as I shall argue, although Weber's philosophical concerns are relevant to his pluralist-elite theory of democracy, they do not, by themselves, account for it.

Weber's Nietzschean Problematic: Bureaucratic Nihilism

When a thinker's philosophical concerns are inexplicit, we can often reconstruct them by looking at their most fundamental problems. Doing so allows us to see what a thinker would have had to presuppose to see them as problems in the first place. Moreover, it also helps us to know what, in their terms, would have counted as solutions. In Weber's case, these problems are already well developed in the literature (e.g., Löwith 1982; Brubaker 1984; Habermas 1984; Schluchter 1981): they have to do with the progressive rationalization of Western culture,

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thought, and institutions. These concerns pervade his writings on religion, law, economics, and politics.

In the case of Weber's political thought, then, we should not be surprised that the classical problems of liberal-democratic theory are of less immediate importance than what he identified as an increasing bureaucratic displacement of politics. He conceived the problem as one of a conflict between the formal (or instrumental) rationality of bureaucracy and the substantive (or value) rationality of politics. In Weber's usage, *instrumental rationality* refers to rationalities of means, the most effective way of achieving a goal. *Value rationality* refers to the rationality of goals themselves, that is, the internal coherence of the interpretive schemas that deal with the intrinsic worthiness of an end. The problem with bureaucracies, for Weber, is that they have no way of embodying value rationality. Bureaucratic rationality is means oriented, merely instrumental. On the other hand, what defines a responsible politics is precisely that political actors take into account the rationality of ends (Weber 1946, chap. 4). Politics ought to make different kinds of value rationalities into objects of discussion, choice, and at some point, power. But where bureaucratic power dominates, responsible politics becomes increasingly impossible (Weber 1978a, app. 2, sec. 2).

Weber's rather Nietzschean way of constructing the issue might be put as follows: in a secular society, a society without values accepted on the authority of faith, bureaucracy is an institutionalized form of nihilism. For Weber, however, it is not—as for Nietzsche—the demise of theological culture and the rise of science in itself that produces a disenchanted, nihilistic universe (Warren 1988, chaps. 1, 7). The disenchantment he identifies in "Science As a Vocation" (1946, chap. 5) is a constant cultural potential, but it is not just this: it is also reproduced and enforced by bureaucratic domination. On the one hand, the external goals of instrumentally rational institutions are likely to have only the most tenuous relation to the goals that individuals pursue in everyday life, goals intimately related to motivation and meaning. On the other hand, the technical efficiency of a bureaucracy depends on training individuals to separate their individual value commitments from their workplace activities. In so doing, bureaucracies forcibly separate individual and institutional goals and combine this separation with a characteristic mode of domination: individuals are regimented, disciplined, and trained for impersonal devotion to the duties of their offices, which are in turn hierarchically structured according to codified rules and procedures (Weber 1978a, 1148–56).

The logic of Weber's argument suggests that the bureaucratic organization of everyday life destroys what he takes to be the secular basis of meaning in activities that are consciously chosen and guided. The cost of bureaucratic efficiency is life in a world with reduced scope for individual autonomy, responsibility, spontaneity, and initiative. Weber in effect borrows Nietzsche's views on how nihilism can affect individuals, but he interprets the problem politically. Nihilism is a result of institutions that systematically separate means and ends, at least where individual actions are concerned. This suggests—at least in principle—a close relation between the structure of institutions and the abilities of individuals to inhabit meaningful universes. As forms of social organization that systematically sever the rationality of means from that of ends, bureaucracies displace the kind of sovereign, responsible individuality appropriate to secular, liberal-democratic societies. In this sense, bureaucracies destroy the social and psychological soil of classical liberal-democratic values, explaining in part why Weber wrote despairingly of bureaucracy as an iron cage.

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Weber's Nietzschean problems presuppose a politically significant commitment to persons as responsible agents, capable of meaningful, goal-directed actions. This ethical commitment provides Weber with a defensible and coherent liberal political philosophy. What keeps these commitments from appearing in Weber's pluralist-elite theory of democracy is that he thought bureaucratic power was inevitable in mass, industrialized societies, and for this reason classical liberal-democratic values could no longer exist, no matter how desirable they might be. The reason Weber's theory of political institutions is such a poor guide to the ethical commitments of his political philosophy is that he thought that contemporary conditions made it impossible to address these commitments politically.

What Weber thought *could* be addressed politically, however, was the closely related problem of organizational drift. Bureaucracies are superior means of achieving a given goal but incapable of determining the rationality of goals themselves. In Weber's terms, bureaucrats are technicians, responsible only for the means of implementing goals that are given and taken for granted. All other things being equal, their experiences and responsibilities train them to disregard the rationality of the organization's goals and the ultimate consequences of these goals. When bureaucratic power displaces political institutions—as they had in post-Bismarck Germany—they make what are in fact irresponsible political decisions under the guise of technical expertise (Weber 1978a, 990–94). In Weber's view, it was precisely such bureaucratic irresponsibility that had allowed Germany to drift into World War I (Weber 1978a, 1385–92, 1407–16). This is why he addressed his leadership theory of democracy to the problem of political leadership in a bureaucratized society while bracketing the deeper problem of bureaucratic nihilism as unsolvable.

Politics As Value Choice and Politics As Conflict

Implicit, then, in Weber's problematic of rationalization, especially bureaucratic rationalization, is a deep commitment to politics in some positive sense, a notion of how political action ought to relate to persons. It is here that one finds a coherent set of liberal commitments in Weber, one for which he ultimately failed to find a political expression. Viewed negatively, politics is essentially "struggle" (Weber 1978a, 1414), the "striving to share power or striving to influence the distribution of power, either among states or among groups within a state" (Weber 1946, 78). Viewed positively, however, politics involves relating individual value rationalities to group choices, implying that recognition of persons together with processes of discussion, argumentation, and consensus lie behind exercises of power. Political actions are a distinctively human kind of social action: they combine instrumental and value rationality, and thus develop and express what Weber conceives as the human potential for teleological action and self-determination. For this reason, political activities are valuable in and of themselves, apart from outcomes.

In "Politics As a Vocation," for example, Weber makes it clear that politics is a meaningful activity only when it involves a control of destiny; only when, to translate into Marxian terms, politics is a historical activity. Political actors may not agree on value commitments, but positive political activities do require that one have value commitments—what Weber refers to as a *cause* or *calling* (Weber 1946, 117). To define politics only negatively, only in terms of conflict, would miss its distinctive qualities and potentials: the "mere 'power politician' may get strong effects, but actually his work leads nowhere. . . . It is a product of a shoddy and superficially blasé attitude toward the

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meaning of human conduct" (Weber 1946, 116–17). Weber's interesting suggestion in this essay is that the archetype of meaningful conduct is political activity carried out in behalf of a cause.

Although he never spells out its ethical dimensions, it seems that for Weber political actions have a distinctive relation to *personality*: politics is the social medium through which individuals can develop and manifest themselves as free, autonomous, and responsible agents (cf. Portis 1983, 33). But there may also be a deeper reason for Weber's attentiveness to the existential situation of political actors. He may be suggesting that it is *only* persons acting in a broadly political sense that can consider and implement value choices in a post-Christian society, at least in any desirable and socially significant way (cf. Weber 1946, 115–28, 148–56). Value choices in a secular society are inevitably political, and only as political beings are humans responsible for value choices and their consequences in practice. Weber's problematic of bureaucratic nihilism thus implies that political activity as such is a distinctively human activity, as well as a desirable replacement for culturally authoritative value consensus. Thus it was partly because Weber saw technocrats taking over value choices as value consensus was breaking down that he took such care to distinguish questions of fact from questions of value. The kind of activity appropriate for value questions is not technocratic expertise, but politics (cf. Weber 1949, 34–38). For Weber, politics is not only a distinctively human activity but also a fate and responsibility (cf. Portis 1986, chap. 3).

Weber's writings combine this underlying sense of urgency about politics in this positive sense with a deep pessimism about its institutional possibilities. This is why the positive dimensions of politics do not find their way into his sociological definitions of political organizations (cf. Scaff 1973). Weber argues that because of the plurality of ends pursued by political organizations, it is not possible to define them in terms of shared values, goals, or commitments (Weber 1978a, 54–55; Weber 1946, 121). Rather, political organizations share only a characteristic means, the use of force. And famously, Weber defined a state as a "human community that (successfully) claims the *monopoly of the legitimate use of physical force* within a given territory" (1946, 78). According to Weber's definitions, then, it would seem that political organizations cannot express the intrinsic good of politics as a form of social action, since they are simply means to a plurality of goods that are extrinsic to the political process itself. Weber even writes that it is our fate that "precisely the ultimate and most sublime values have retreated from public life" (1946, 155; cf. Weber 1949, 55). This is why, for Weber, politics today must inevitably be a power struggle over a multitude of ends; why value questions affecting society as a whole must ultimately be decided against a background of coercion. Thus the positive and negative dimensions of politics remain polarized in Weber's political philosophy: positive politics, broadly conceived as all social actions oriented toward determining one's future together with others, is a distinctively human good that is necessary in a secular society. Yet it is precisely politics in this sense that increasingly lacks institutional means of expression. What I am arguing, then, is that the most compelling problem of Weber's political philosophy is this conflict between an ethically significant sense of politics and contemporary institutions.

From this perspective, it becomes clear that commentators who argue about reconciling the three most obvious political values in Weber—his liberalism, his nationalism, and his elitism—miss the problem. These values are not of similar conceptual status or origin in Weber's thought, and to miss this is also to miss the way the inconsistencies within his

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thought mirror a political predicament—one that with modifications, turns out to be ours as well. What we find in Weber is an attempt to reconcile his liberal commitments—to personhood, freedom, and the parliamentary government and competitive capitalism that presumably make these possible (Beetham 1985, chap. 2)—with an illiberal political reality and capitalism that was no longer competitive. In the process, he modifies his liberal commitments to the point that they are minimally expressed in his theories of political institutions.

Weber's nationalism and statism are not necessarily inconsistent with his liberalism. As David Beetham (1985, chap. 5) and Edward Portis (1986, 121–24) have convincingly argued, the nationalism of his middle and late writings stemmed from his view that people can develop into distinctive personalities in only communal contexts defined by distinctive cultures. Nor was Weber's nationalism exclusively German, although, as Beetham also points out, his cultural orientation remained Eurocentric. In principle, however, he regarded all historically evolved nationalities as worthy of protection. His concern for the German state was an outgrowth of his belief that under contemporary conditions, the survival of national communities depended on their organization into states, or at least having political power within states. Germany's "world responsibility," in Weber's view, was to protect the national communities of central Europe from Anglo-American and Russian domination. Weber's nationalism and statism, then, are not sui generis but rather interdependent with his liberal commitment to persons.

Finally, Weber was an elitist and not a democrat except in the minimal sense of his pluralist-elite model of democracy, a point to which I return. He held that in bureaucratized societies only a few are capable of living up to his ideals of individual freedom, responsibility, and autonomy. The resulting elitism was, I will argue, an outgrowth of Weber's liberal commitments unleavened by democratic ones, and narrowed by his views on the inevitability of monopoly capitalism and bureaucratized government. It is in this context that his general concern with persons becomes a specific concern with the autonomy and responsibility of elites with respect to bureaucratic organizations (Beetham 1985, 57–58). For this reason, Weber's elitism should be accounted for by his analysis of political possibilities and not as a separate kind of political commitment.

The tensions between Weber's value commitments, then, are interesting mostly as symptoms of the deeper conflict between an ethically significant sense of politics and contemporary institutions. I now deal with the implications of this conflict by looking at two questions. First, Is Weber's commitment to a positive concept of politics a plausible ethical response to what I am calling his politicized *Nietzsche problem*, the collusion of secularization and bureaucratization domination that undermines a meaningful sense of politics? The reconstruction for which I argue would have led Weber in a substantially democratic direction, had he fully conceptualized his commitments. Second, Was Weber's elitist elaboration of his commitments the only political possibility, given the nature of mass, industrial societies? I shall argue that clarifying Weber's commitments also clarifies some political possibilities that he did not entertain.

Weber and Kant: The Political Ethics of Personality

With regard to the first issue, commentators often suggest that Weber's liberalism includes an ethical relativism that furthers the modern reduction of right to

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power (Eden 1984; Strauss 1950, chap. 2). Support for this kind of position comes from Weber's observation that choices between values are inevitably individual and subjective. Rationality can clarify value choices, he argues in "The Meaning of 'Ethical Neutrality,' " but cannot provide universal grounds for making them (Weber 1949, 17–19). Each must choose his or her own god or devil, and this is what it means to live in a secular world (Weber 1946, 151–53; Brubaker 1984, 98–101).

If by *relativism*, however, these commentators mean that Weber sees no ways of distinguishing better and worse actions, then they have misinterpreted his position. Indeed, he protests that to understand his comments in this way would be the "crudest misunderstanding," arguing that they lead instead to a "very special type of ('organic') metaphysics" (1949, 18). Unfortunately, he says little about what this special type of metaphysics might be.

Nonetheless, Weber's position seems to include two interrelated claims. Habermas (1984) has elaborated the first in his *Theory of Communicative Action*. On Habermas's account, Weber does not seek one set of value criteria to ground all others because he sees cultural progress in the differentiation of value spheres, none of which are primary and each of which has its own distinctive kind of rationality and criteria of propriety. This is true for economics, aesthetics, erotic life, language, intellectual life, social life, and so on. Each sphere is necessary for a fully human life, and it would be inappropriate to universalize the standards of one sphere to all others. One does not judge art by logical consistency, love by utility, or righteousness by efficiency. To demand that all value spheres reduce to one would be to empty life of its substance, to flee from life into a world of formal and lifeless standards (Weber 1949, 17), a point that has been revived recently in ethical philosophy (cf. Williams 1985). At the same time, however, Weber does not suppose that these spheres of value will necessarily coexist in harmony. One is often faced with absolute choices between value rationalities, and this is why one who is fully conscious of his or her fate will know that there are no compromises between God and the Devil (Weber 1949, 18).

Yet Weber's position is not a simple relativism, because it is based on a view of what humans are, such that their lives could be simultaneously rooted in differentiated and autonomous spheres of value. This is presumably where one finds the special kind of organic metaphysics that he alludes to but does not develop. Weber's commitment to a specific conception of human activity becomes clear in a second claim, and this is what ultimately gives ethical content to his notion of political action. In the same paragraphs in which he writes of the inevitable plurality of values, he also argues that humans have an imperative to "choose" between values when they conflict rather than live semiconsciously and muddle through. "The fruit of the tree of knowledge," he writes, "which is distasteful to the complacent but which is, nonetheless, inescapable, consists in the insight that every single important activity and ultimately life as a whole, if it is not to be permitted to run on as an event in nature but is instead to be consciously guided, is a series of ultimate decisions through which the soul—as in Plato—chooses its own fate, that is, the meaning of its activity and existence" (Weber 1949, 18). At one level, of course, Weber's emphasis on decisions seems to support the idea that standards of conduct are merely subjective. At a deeper level, however, his imperative clearly reflects a Kantian conception of persons, a view that human dignity resides in the ability to choose consciously, through which one manifests oneself as a free and rational agent (cf.

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Brubaker 1984, 95–101). This is not surprising, since the academic atmosphere of Weber's Germany was permeated by neo-Kantian ethics and epistemology, a fact often overlooked in Anglo-American commentary.[2] It is in terms of this background that the ethical commitments of Weber's political philosophy begin to emerge.

Kant's approach to freedom was distinctive because of his equation of reason, moral choice, and freedom: freedom is our capacity to use our reason to guide our behavior. As Charles Taylor (1985, 318–21) has pointed out, what proved attractive about Kant's understanding of freedom when compared to earlier liberal thought is that he radically affirmed self-determination, but without either privatizing the self, or interpreting freedom as the victory of impulse over reason. Kant's moral schema thus shifted liberal thinking away from natural-rights approaches, and toward a focus on how value choices constitute humans as rational agents, especially with regard to the way moral choices are manifested in actions. Kant opposed moral schemas that prescribe values—whether by means of religious or metaphysical dogmas or through various kinds of social utilitarianism that ultimately require institutions to distribute values. Such external prescriptions violate the individual's ability to choose freely. In the case of moral values, actions cannot be said to have moral worth unless they result from free choice. Because for Kant humanity is defined by the capacity for moral choice, schemas and dogmas that equate moral action with prescribed behavior violate the personhood of individuals (Kant 1970, 73–86). Weber's concept of personality follows these broadly Kantian lines of reasoning: like Kant, Weber finds an ultimate value in freedom construed as rational agency (or *personality*, in Weber's terms). And like Kant, he construes practical reason (moral choice) as a condition of this kind of freedom (Weber 1949, 15–18; Löwith 1982, 43; Schluchter 1979).

One might still argue, however, that Weber's position is implausibly relativistic because he believes—unlike Kant—that a commitment to a rational teleology of personhood is, in the end, a matter of faith (Weber 1949, 16–17). But what "faith" means in this context is simply that all arguments involve axiomatic premises that cannot themselves be rationally demonstrated. What separates Kant from Weber is that Kant held that his premises were exhaustive, and that this commits all humans to them insofar as they are potentially rational beings. Weber simply avoids universalizing his claim because he understands that one cannot argue rationally for the axiom that rational capacities define a fully human life. To do so would be to presuppose what one is trying to demonstrate.

Still, one might make the case (I will not here) that while Weber's axioms are a "matter of faith" in that they are (formally) a priori, they have the quality of all good axioms in that they are manifested in the activity of rational argumentation itself. Such an argument would follow Habermas's development and critique of Weber in *The Theory of Communicative Action* (1984) from the perspective of his "discourse ethics." On Habermas's account, the formally binding quality of the premise resides in the *process* of argument about moral commitments and not in a prior agreement about their meaning and content. Thus, speaking implies a commitment to the personality of others qua rational agents. Without this premise, there is nothing more to be said, except to note the empirical fact of different value rationalities. This may be (and almost always is) a political problem, but this should not be confused with the formal structure of Weber's commitment to personality nor with the implied universality of these commitments. It is in this context, then, that one should under-

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stand Weber's argument that value choices *as such* and the "belief in the validity of values" remain necessary for the possibility of personality as something developed in and through actions. This is so even if, for Weber, the validity of value choices remains "subjective."

What is important to notice, then, is that what is not subjective for Weber, and hence not relativistic, is the possibility—indeed, the existential and moral necessity—of subjectivity itself. "Certainly the dignity of 'personality,' " he writes, "lies in the fact that for it there exist values about which it organizes its life;—even if those values are in certain cases concentrated exclusively within the sphere of the person's 'individuality,' then 'self-realization' in those interests for which it claims *validity* as *values*, is the idea with respect to which its whole existence is oriented. Only on the assumption of the belief in the validity of values is the attempt to espouse value-judgments meaningful. However, to *judge* the *validity* of such values remains a matter of faith" (Weber 1949, 55). What Weber is saying here is that although the specific content of value judgments is "a matter of faith," *as humans* we share an interest in a meaningful existence (cf. Weber, 1946, 275). Realizing a "meaningful" existence depends on orienting actions in terms of value judgments, that is, "organizing one's life" in relation to values. "Personality" is the result of a life so organized. This claim, with its implicit moral teleology, is part of Weber's ontology of social action (or, as he puts it, his "special kind of ['organic'] metaphysics") and part of his understanding of what humans are.

It is this broad claim about the imperatives of value choice that Weber construes in terms of an axiomatic commitment to rationality. Thus his normative definition of personality focuses on the "consistency of its inner relationship to certain ultimate values and meanings of life, which are turned into purposes and thus into teleologically rational action" (Weber 1975, 192; cf. Löwith 1982, 45–46; Portis 1978, 113–20; Schluchter 1979, 73–74; Weber 1946, 151–53). In this way Weber equates meaningful action with what is potentially rational, in the sense that one who is living a meaningful life can, in principle, provide a consistent account of his or her actions in relation to both values and circumstances (Schluchter 1979, 76–92), in this way combining instrumental and value rationality in practices. His concept of rational agency requires not only that individuals make conscious choices between the demands of conflicting value spheres but also have a clear understanding of the means and contingencies of action.[3] The political significance of these points is that they imply that persons' capacities for rational agency are intrinsically valuable and therefore ought to guide and limit political judgments.

Although Weber's ethics develops in the spirit of Kant in these respects, he departs from Kant in another. While Kant holds that freedom implies the universalizability of maxims of conduct, Weber's universal commitment attaches to the inner consistency of values and their integration with empirical conditions of action. His departure from Kant in part stems from his view that there are many possible value rationalities, each closely related to its cultural context. This point reminds us of Hegel's critique of Kant's categorical imperative as implausibly formal. But equally importantly, it represents Weber's emphasis on the consequences of action. There are a multitude of worldly situations, especially in politics, in which there are no unambiguously right courses of action. As Weber's contrast between an "ethics of responsibility" and an "ethics of ultimate ends" in "Politics As a Vocation" suggests, Kant's universality is possible only in intentions, not outcomes (Weber 1946, 118–27). Those who would act according to an ethics of ultimate ends (Kant is only one example)

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act only on the basis of the inner consistency of value systems. They consider any action "good" only to the degree that it reflects value-rational maxims of behavior. But value-rational intentions are not sufficient to rational action because one can hold them without regard to the contingencies and consequences of action. When one fails to link intentions to worldly consequences in an instrumentally rational way, good intentions can and often do produce quite contrary results. "Ethics of ultimate ends"—that is, ethics that are strictly intentional and value rational—are not sufficient to the teleologically rational action that defines personality. An "ethic of responsibility," on the other hand, requires that persons strive for long-term consistency between value commitments and outcomes, a consistency that develops and manifests personality.

Karl Jaspers has suggested, correctly I believe, that the effect of Weber's appropriation of Kant was to deemphasize formal rules of conduct and draw out the responsibility for consequences implicit in Kant's formulation of the moral question. He comments that the

> opposition between principle and results as a basis of ethics is not tenable. The ethics of principle itself demands that in concrete action guided by the categorical imperative, one should bear the consequences in mind. The true basis of ethics is a third factor, which Kant, who obviously has it in view, does not expressly mention but which Max Weber brings out clearly in his 'ethics of responsibility.' Ethics of responsibility is the true ethics of principle. It does not take mere results or rational principles as its guide but seeks its way in the open arena of possibility, pursuing an absolute that is not manifested through any material content, but only through thought in actions. (Jaspers 1962, 72–73)

Weber in fact makes essentially the same point with respect to Kant in "The Meaning of 'Ethical Neutrality' " (1949, 16–17).

Weber emphasizes, however, that an ethics of responsibility is not a recipe for right action, especially in politics. Actors will inevitably find themselves confronted with ethical conflicts between means and ends. That the world has amoral qualities means that politics will usually have a tragic dimension. Weber's ideal personality would be conscious of this element of the human condition and accept it without paralysis or cynicism: "It is immensely moving when a *mature* man—no matter whether young or old in years—is aware of a responsibility for the consequences of his conduct and really feels such responsibility with heart and soul. He then acts by following an ethic of responsibility and somewhere reaches a point where he says: 'Here I stand; I can do no other.' That is something genuinely human and moving" (1946, 127).

It is in remarks such as these that one finds Weber's radical reappraisal of politics: no longer is it simply a necessary evil, but rather it is a positive and definitively human activity (cf. Löwith 1982, 47). Politics is the most demanding test of an ethics of responsibility, and at the same time it is the kind of activity through which personality is cultivated and manifested as the expression of responsibility. Insofar as politics has this potential within the human condition, it is valuable in itself. It is this morally significant sense of politics that guides and structures much of Weber's thinking and no doubt accounts for much of his concern with the bureaucratic displacement of politics.

Weber's Commitment to Persons: Elitist or Democratic?

Yet what is problematic in Weber is whether his politicized, neo-Kantian ethic could, in principle, be expressed in political institutions. In this respect Weber's departure from Kant remains significant. Kant's position on this issue was clear: there is no *necessary* conflict between individual freedom and agreement on uni-

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versal standards of conduct, that is, between practical reason and political right. What morality requires of individuals is not agreement on external goods and values but rather agreement that the choices people make not involve treating others only as means. Kant's rule for accomplishing this was, of course, the *categorical imperative*: the notion that the guiding maxim of an act ought to be put to the test of universality. In principle, Kant (1964; 1970, 132–33) considered the respect for persons as free and rational agents expressed by the categorical imperative to be a possible, sufficient, and intrinsically worthy goal for a liberal society.

It is here that one finds a genuine political indeterminacy in Weber's ethical position. In contrast to Kant, Weber simply avoids universalizing his commitment to persons as rational agents, even within societies. For this reason, his commitment to persons produces an elitism rather than the liberal-democratic position that follows from Kant. What one finds in Weber is a heroic interpretation of personality: personality requires a strength, intelligence, commitment, and realism that he believed only a few in society could ever achieve. Combining this heroic interpretation with the notion that hierarchies of domination are inevitable in any society, Weber arrives at what Brubaker (1984, 97–98) correctly describes as an aristocratic morality, one that is only very marginally liberal and not at all democratic. In "The Meaning of 'Ethical Neutrality,' " for example, Weber writes that "only one thing is indisputable: every type of social order, without exception, must, if one wishes to *evaluate* it, be examined with reference to the opportunities which it affords to *certain types of persons* to rise to positions of superiority through the operation of the various objective and subjective factors" (1949, 27).

How fundamental are Weber's claims here? If I am right about the structure of Weber's ethical commitments, his elitism is one possible elaboration of his commitment to persons. This would mean that the elitism is not fundamental. Instead, it is all that is left of his liberal ethics, with its respect for persons and its ethically significant sense of politics, when he combines it with an analysis of the realities of bureaucratic domination in mass, industrialized society. If so, an originally liberal and substantially democratic political ethos would have been overtaken by Weber's assessment of the realities of power in modern society, producing a marginally liberal elitism.

The point of my argument is not, of course, somehow to save Weber from himself. Rather, it is that looking at Weber in this manner makes him interesting in a way that he would not be if his elitism were simply a matter of taste and personal antipathy for—and perhaps fear of—the masses, as Beetham, for example, suggests (1985, 111–12). This approach shows how Weber's narrowing of his ethical commitments to persons represents what was certainly a political reality in his day and remains so in our own, namely, a stark contrast between liberal-democratic commitments to persons, and the power relations within liberal-democratic societies that undermine the social conditions of these commitments. Reading Weber in this way allows us to learn something about ethical dilemmas of liberal-democracies in mass-administered societies.

In order to define more clearly what is at stake, let us perform the thought experiment of separating Weber's ethics from his analysis of political possibility. What would a good society look like then? A good society would treat politics as something essential to the personhood of its members, assuming we understand politics to include all activities relating to rational agency in social situations. If, therefore, we were to define a good society solely in terms of Weber's ethical com-

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mitments to persons, we would have defined its desirability in terms of its substantially democratic aspects. Only this kind of society would be desirable in a Nietzschean world where there are no transcendental sources of value, because only such a society would allow individuals to give a meaningful durability to the world of their everyday activities. Such a society would not, of course, be devoid of conflict, tragedy, and violence. Nonetheless, as Weber himself points out (1949, 26–27), although social conflict is inevitable, there are many ways for it to be peacefully expressed. The problem for a society that would express Weber's value commitments, then, would be, first, to institutionalize the respect for persons within large-scale institutions without imposing agreement. Second would be to maximize dialogical possibilities of agent formation within small-scale ones (cf. Macpherson 1977, chap. 5; Mansbridge 1981; Habermas 1979, chap. 3). In this way, Weber's problematic of bureaucratic nihilism and his political ethics produce an imperative for substantive democracy quite at odds with the pluralist-elite model of democracy he actually advocated.

Pluralist-Elite Democracy and Democratic Values

When Bismarck's rule came to an end in 1890, he left, in Weber's words, a nation *"without any political sophistication"* (1978a, 1392; cf. also 1385–91, 1407–16). It was in the context of a post-Bismarck Germany that had drifted into world war that Weber developed his pluralist-elite model of democracy. His outstanding concern was with the bureaucratic displacement of politics and domination of technocrats. The outstanding features of the model were, first, a strong, even Caesaristic political leadership based on the plebiscite and limited by parliament. Second, Weber expected that this formula for leadership would produce an equilibrium of countervailing forces between political leadership and bureaucratic power, an equilibrium that would ensure a relatively open and dynamic society (Weber 1978a, 1414–17; Beetham 1985, chap. 4).

It is to be expected, then, that commentators on Weber's views of democracy would focus almost entirely on the question of leadership and elite competition. Karl Jaspers, for example, writes that the "primary concern of his political thinking was how to create an active harmony between democracy—which was inevitable anyway—and the authoritative leadership of truly able and responsible statesmen" (1964, 201). Wolfgang Mommsen who, unlike Jaspers, is critical of Weber, notes that he "was an advocate of democracy on the grounds that, under the social and political conditions of a modern bureaucratic society, it offered a maximum of dynamism and leadership. The classical doctrine, however, meant little to him. He did not believe in the theory of the sovereignty of the people" (1974, 87; cf. Kilker 1984). Weber saw the democratic process as a "competitive struggle of various political leaders for the support of the people." In this sense, Mommsen points out, Weber's theory of democracy is clearly the "liberal model of competition" transplanted "from economics to the field of parliamentary mass democracy" (1974, 88; cf. Macpherson 1977, chap. 4). Similarly, David Beetham notes that Weber originated the "theory of competitive elitist democracy that gained such widespread acceptance within political science in the 1950s and 1960s" (1985, 2).

From the perspective of Weber's political ethics, his view of democracy as a mechanism for leadership selection would seem to be quite consistent with his neo-aristocratic view that societies should be judged in terms of the opportunities they

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afford for superior leadership (Weber 1949, 27). Weber himself does not even attempt to justify his model in democratic terms. He does so instead in terms of liberal, elitist, and nationalist values (Beetham 1985, chap. 4). Through checks and balances, it would limit the power of government to interfere with individual liberties. By providing an arena of political competition, it would select for the most able leaders. Finally, it would provide the German state with the capacity to assume a place in world politics, allowing it to protect its own national culture as well as those of the weaker nations of central Europe.

It would, in fact, have been much more difficult for Weber to have justified this model in terms of his political ethics of personhood with its substantially democratic implications. That this is so can be seen from Weber's own assessment of the plebiscitary basis of leadership. He insists that if leadership is to have any democratic meaning whatsoever, it would follow from the fact that legitimacy is "*formally* derived from the will of the governed" (Weber 1978a, 268). Even pluralist-elite democracy rests on the voluntary and autonomous consent to the leadership of particular individuals. That this is important to Weber's model becomes clear when he defines democratic legitimacy in the first part of *Economy and Society* as an antiauthoritarian species of charismatic legitimacy, writing that the "basically authoritarian principle of charismatic legitimation may be subject to an anti-authoritarian interpretation, for the validity of charismatic authority rests entirely on recognition by the ruled, on 'proof' before their eyes. . . . When the charismatic organization undergoes progressive rationalization, it is readily possible that, instead of recognition being treated as a consequence of legitimacy, it is treated as the basis of legitimacy: *democratic legitimacy*" (1978a, 266–67).[4]

Although Weber only intended his description of democratic legitimacy to elaborate a sociological category, his definition turns on a distinction between authoritarian and democratic charisma that has a normative importance. According to Weber's definition, one should speak of democratic legitimacy only where recognition of a leader *precedes* the relationship of legitimacy. The democratic content of Weber's pluralist-elite model of democracy, then, is in the fact that democratic legitimacy presupposes a more or less considered decision-making process on the part of the governed.

In the context of Weber's broader sociology, however, one finds that even this minimally democratic substance—consent to being ruled by another—would have to exist under very special circumstances if it were not to be merely a formal appearance of democracy. *Voluntary consent*, *autonomy*, and even *will*, imply a consciously considered choice. But the status of "choosing" in Weber's model is tenuous. From the perspective of his sociology of mass, industrial societies, it is precisely the existence of individuals as rational agents capable of making choices that is problematic. Weber did not, like many liberals, believe that individuals are necessarily rational utility maximizers. As a sociologist, he observed that most mass followings of leaders involve "purely emotional and irrational" factors (1978a, 1459). Moreover, Weber noted that such followings are especially characteristic of modern mass democracies. It was for this reason that he fully expected plebiscitary leaders to use demagogic techniques to solidify their followings. Very often such leadership evokes mass behavior—something Weber declines to describe as "social action" because of its lack of intentional character (1978a, 23).[5] This is why he thought that the consent elicited by a plebiscitary leader would in fact be "only formal and fictitious" (1978a, 267).

My aim is not, of course, to fault

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Weber's descriptions of plebiscitary leadership from a sociological perspective. The point here is that if Weber's sociological considerations are correct, then they blur the limited sense in which it makes sense to describe such political processes as democratic. Surely Weber was aware of this, lending credence to Mommsen's suggestion that for Weber "democratic institutions are a merely functional machinery" in the hands of charismatic leaders, pushing him dangerously close to the "fascist leadership principle" (1974, 93). Although Mommsen may overstate the case, he is right that from the perspective of substantial democracy (as opposed to Weber's own emphasis on the formal mechanisms of democracy) it is very difficult to distinguish between voluntary consent and mass behavior. This is why Mommsen can plausibly argue that Weber fails to distinguish between charismatic leadership and charismatic domination, on which the difference between liberal-democratic leadership and demagogic dictatorship presumably depends. Similar considerations have led Jürgen Habermas to characterize Weber's concept of leadership democracy as a somewhat desperate attempt to break the powers of bureaucratization by means of a "decisionist self-assertion in the midst of a rationalized world" embodied in a strong-willed leader's instinct for power (1971, 65–66).

Clearly there is little conflict between this institutional model and Weber's elitist and nationalist sentiments. But it is also clear that the model gives little or no expression to the ethical commitments of his political philosophy, those I have characterized as based on a commitment to persons and a positive conception of politics, oriented toward bureaucratic nihilism and having substantially democratic implications. Thus it remains to be asked whether Weber's pluralist-elite model was the *only* way he could have institutionalized his political ethics.

Limits to Democracy: "The Principle of Small Numbers"

As J. J. R. Thomas (1984) has recently argued, it was not for lack of considering possibilities of direct democracy that Weber took the position he did. The fact that he considered and rejected at least some substantially democratic alternatives makes his position all the more challenging for democratic theory. His argument against substantive democracy was based on two claims: the first and most obvious problem is the size of modern societies; the second and more compelling problem has to do with bureaucratic power. But while Weber's "impossibility" arguments are crucially insightful, especially in the latter case, they are not decisive.

With respect to the question of size, Weber held that direct democracy is possible only in small, relatively undifferentiated and simple societies. The problem with democratizing political decisions is that actual political decision making is always governed by the "principle of small numbers," which expresses the "superior political maneuverability of small leading groups" (1978a, 1414). The tendencies toward exclusiveness in political decision making grow as societies become larger and more complex, suggesting that under modern circumstances direct democracy is impossible.

Does this mean that, in Weber's terms, substantive democratic values can have no place in modern societies? Although this limitation may relate to *direct* democracy, it does not so clearly apply to less demanding forms of *participatory* democracy. Weber's positive political ethos would not require that every individual participate in every political decision. What it would require is that individuals participate directly in those decisions that most immediately affect their everyday activities. The reason is that, in Weber's

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terms, politics can have positive qualities only where it relates to the "local" development of persons, and to their senses of freedom and self-determination (cf. Weber 1946, 155). Thus, while direct participation in foreign-policy decisions may not be feasible for large numbers of individuals (one of Weber's major concerns in "Parliament and Government in a Reconstructed Germany"), it also has less immediate importance for a positive politics than, say, power in the workplace, neighborhoods, consumer cooperatives, and the like. On the other hand, representative mechanisms would be both feasible and sufficient in those larger-scale decisions that very often determine the contingencies of everyday life, including foreign-policy decisions, investment and production decisions in large-scale industries, decisions on macroeconomic policy, and so on. What Weber's political ethics demands, in other words, is participation appropriate to the level of organization at which decisions are taken. This is the option that he does not consider.

That Weber did not seriously consider these kinds of participatory options shows up in the fact that when he considers socialist principles of economic organization, he looks only at "cartels of producers," and "cartels of consumers," both of which would require centralized state control to be effective (1978b, chap. 12, cf. Weber 1978a, 1454). What Weber fails to consider in this context is democratization of production and other spheres of society in ways that would extend opportunities for participation beyond the spheres of government as they are defined in liberal societies. He does not do so, it seems, because he sees the increasing bureaucratization of all spheres of society as inevitable.

Limits to Democracy: Bureaucracy

Weber's more compelling argument, then, has to do with the inevitability of bureaucratic power in mass, industrialized societies: "As soon as mass administration is involved, the meaning of democracy changes so radically that it no longer makes sense for the sociologist to ascribe to the term" *democracy* the meaning it has in situations of direct democracy (Weber 1978a, 951). Weber's argument includes three kinds of reasons why bureaucracy limits democracy.

The first has to do with conflict between government by law and democratic control. Weber held that there is an intrinsic tendency toward bureaucratization with the rise of democratic demands for rational and consistent treatment by government (Thomas 1984). Democratic legitimacy minimally involves equality before the law and hence a legal-rational structure of rules. Administering these rules requires that some body of individuals have powers of interpretation and enforcement, which in turn leads to legal-rational domination. Yet " 'democracy' as such is opposed to the 'rule' of bureaucracy, in spite of and perhaps because of its unavoidable yet unintended promotion of bureaucratization" (Weber 1978a, 991). Weber's insight into the development of formal democratization is that insofar as it involves a "leveling of the governed in the face of the governing and bureaucratically articulated group," the bureaucracy may "occupy a quite autocratic position, both in fact and in form" (1978a, 985; cf. Cohen 1972, 72–77). It is precisely this "passive" democratization that leads to the tension between democracy and bureaucracy. At best, Weber believed the effects of rationalized administration could be stemmed through "political democratization," by which he meant those constitutional mechanisms designed to prevent "the development of a closed status group of officials in the interest of universal accessibility of office," and to minimize "the authority of officialdom in the interest of expanding the sphere of 'public opinion' as far as practicable" (1978a, 985).

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The problem of rule-based administration is compounded by a second problem, namely, the nondemocratic character of expertise. Here, the issue is not, as in the first case, one of an internal contradiction within democracy, but the more familiar one of a trade-off between the advantages of a technological society and the good of self-government. Where "a group of specialists is present," Weber writes, "no matter how strongly the attempt is made to keep them in a dependent position, the seeds of bureaucratization are present. Above all, such persons can neither be appointed nor dismissed according to the procedures appropriate to immediate democracy" (1978a, 290).

Yet in both cases one can imagine ways of arranging complicated societies so that powers of administration and expertise are checked and divided so as to make them less threatening to democracy. The question is not whether authorities should exist, but whether they transgress the boundaries appropriate to the kind of authority they possess. On this principle, I may be subject to the authority of someone else in one sphere of existence (for example, to a policeman's legal authority when I am driving, to a doctor's expert authority when I am sick, and so on). But *I* might be an authority in other spheres, and an equal in spheres that permit equality. Indeed, the public spheres of liberal-democracies already embody some of these distinctions. The point would be to extend them to bureaucratic organizations throughout society, assuming ways could be found to make them effective. Thus, the overall effect could be a society that is highly differentiated and pluralistic, and yet relatively equal with regard to the values of personhood (cf. Walzer 1983). Certainly Weber's pessimistic image of bureaucracy as an "iron cage" cannot be based on the intrinsic properties of rules and expertise alone.

Rather, what is decisive for the conflict between bureaucracy and democracy is a third factor: power. Bureaucracies provide technically superior means of control. This quality of bureaucratic organizations—for Weber as much as for Foucault—is the deepest and most intractable problem for substantive democracy. According to Weber's analysis, both economic and political processes cause bureaucratization in modern societies. In capitalist economies, bureaucracy spreads simply because profit is closely related to its technical superiority in production. Where government is concerned, bureaucratization is encouraged by demands for equal treatment and uniform services. The forms of administration that result produce a "passive democratization," combining a formal equality in the delivery of services with higher degrees of control by organizations over both their employees and clientele (Weber 1978a, 949, 985–86).

The superior technical capabilities of bureaucracy, however, not only provide superior means of power, but also result from preexisting relations of power. In such cases, the split between individual and institutional goals does not simply reflect imperatives of efficiency; it also reflects—indeed, presupposes—the power of some individuals to enlist others in the service of their goals (cf. Weber 1978a, 289–92). Weber is quite clear about the fact that bureaucratic domination is fueled in both capitalist and (state) socialist societies by the manner in which groups of individuals monopolize social resources (1978a, 1394). This is why they have the ability to use others as instruments in rationalized schemes of administration and production. In language that evokes Marx, Weber writes that

> the hierarchical dependence of the wage worker, the administrative and technical employee, the assistant in the academic institute *as well as* that of the civil servant and the soldier is due to the fact that in their case the means indispensable for the enterprise and for making a living are in the hands of the entrepreneur or the political ruler. . . . The "separation" of the worker from the

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> material means of production, destruction, administration, academic research, and finance in general is the common basis of the modern state, in its political, cultural, and military sphere, and of the private capitalist economy. In both cases the disposition of these means is in the hands of that power whom the *bureaucratic apparatus* (of judges, officials, officers, supervisors, clerks, and noncommissioned officers) directly obeys or to whom it is available in case of need. This apparatus is nowadays equally typical of all those organizations; its existence and function are inseparably cause and effect of this concentration of the means of operation. (Weber 1978a, 1394; cf. Weber 1978b, 253)

Weber's comments here are important, for they suggest that he was quite aware of what critics such as Marcuse (1968, chap. 6) accuse him of overlooking, namely, that instrumental rationalization —whether in capitalist or state-socialist societies—is supported by power relations stemming from control over social resources. Bureaucratization is not, then, simply an effect of the technological requirements of a complex society, nor even less an inevitable social expression of "rationality." What Weber is claiming is that bureaucracies both presuppose power relations based on control over means of existence, and provide superior means of increasing control. His pessimism regarding the march of bureaucracy stems from the fact that bureaucratic organizations are superior means of power, and this makes it less likely that concentrated control can be successfully challenged. The key to criticizing Weber on this point, as Habermas has pointed out (1984, 220–22, 241–42), is to notice that Weber equated the rationalization of society with bureaucratization and did not entertain the possibility of any other *institutional* forms of rationalization. This made it difficult for him to locate rational alternatives. This is so in spite of the fact that Weber's conception of rationality in social life is very much broader than the instrumental rationality of bureaucracy.

Nonetheless, the importance of this point is that it shows the *political* nature of Weber's problem; that the discontinuity between his political ethics and his pluralist-elite model of democracy reflects his assessment of political reality, and not just problems of principle. It suggests that if solutions exist to the problems of bureaucratization, they would have to be political in nature and address the distribution of control over social resources throughout the institutions of society. In terms of Weber's logic, only this kind of redistribution would remove the sustaining cause of bureaucratization. This would, of course, involve a substantial democratization of society, consistent with Weber's positive political ethic. This would be the clearest imaginable solution to his politicized "Nietzsche problem," bureaucratic nihilism. Yet any workable solution requires a political agent. Weber saw only a few courageous individuals, but no political movements likely to challenge bureaucratic domination. This is presumably why he opted for a position that could remain intact politically, if not as a liberal-democratic political philosophy.

Conclusion

Weber does have a coherent political philosophy, then, one concerned with individual capacities to construct meaningful lives in an increasingly secularized and bureaucratized world. Yet he is inexplicit about the values of his political philosophy, in large part because he sees little or no possibility for their institutional expression. This leaves the underlying problems of his political philosophy unsolved, as we see in the conflict between his political ethics and what he took to be a possible politics—his pluralist-elite theory of democracy. This conflict in Weber between an ethically significant sense of politics and its institutional possibilities is compelling because it

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is also our conflict: it reflects a continuing divergence between the promise and performance of liberal democracies. At the same time, framing Weber's problems in these terms allows us to identify solutions that remain obscure in Weber because he does not always distinguish between problems of principle and those of power in his analysis of bureaucracy. Identifying the power relations that sustain bureaucratic domination suggests that Weber's problem of a meaningful politics in a secularized world, could, at least in principle, be addressed through substantive forms of democracy now lacking in liberal-democratic societies.

Notes

For their insightful comments and criticisms, I am indebted to William Connolly, Thomas Dumm, Stuart Gilman, Asher Horowitz, Alkis Kontos, Jane Mansbridge, Edward Portis, and J. J. R. Thomas.

1. I intend the term *pluralist-elite theory of democracy* to refer to the model explained and developed by C. B. Macpherson (1977, chap. 4). Macpherson refers to this model as "equilibrium democracy" because of its emphasis on marketlike equilibrium outcomes of elite competition.

2. Some aspects of Weber's Kantian legacy are, of course, better known than others. He subscribes to the Kantian distinction between moral choice and empirical existence, for example, when he insists that values are subjective commitments that cannot be scientifically (that is, empirically) validated. Likewise, Weber follows Kant in pointing to the nonscientific character of value judgments, while also—like Kant and against the positivists—holding that values have a rational meaning. Much of Weber's writing on the philosophy of social science was aimed at saving value choices from pseudoscientific claims, a central point of "The Meaning of 'Ethical Neutrality' " (1949, chap. 1).

3. Indeed, for Weber, the ethical significance of social science is its utility in clarifying conditions of rational agency. His choice of objects (social action) as well as his choice of methods (interpretive reconstruction of subjective orientations) both follow from his interest in personhood. His social science is appropriate to the study of humans insofar as they are conceived as agents, rather than, say, organisms manifesting behavior. Wolfgang Schluchter alludes to this point when he notes that for Weber "the *conceptually presumed* consistency of personality is a kind of transcendental precondition of interpretive sociology" (1979, 73).

4. In Weber's schema, the plebiscitary leader is "the most important transitional type" between authoritarian charismatic domination and government where choice has become rationalized in an "elected officialdom" (Weber 1978a, 266–67). Thus, although Weber would not consider a plebiscitary democracy to be a mature democracy, the rationalization of this mode of legitimacy adds nothing to its consensual basis, and can be treated in the same way in regard to this issue.

5. Much like contemporary pluralists, Weber concluded that to counter the "direct and irrational mob rule typical of purely plebiscitary peoples," it is desirable that individuals be represented by means of secondary groups, such as clubs and unions as well as political parties (1978a, 1460).

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Mark Warren is Visiting Assistant Professor of Political Science, Northwestern University, Evanston, IL 60608.

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POLITICAL SCIENCE AND THE ENLIGHTENMENT OF ENTHUSIASM

JAMES FARR
University of Wisconsin

I provide a narrative of the emergence of an expressly articulated "political science" in the Scottish Enlightenment. Political science was designed by Hume, Smith, and others to advance both a Newtonian method for the study of politics and a politics of moderation whose tasks included a critique of enthusiasm. In this way, political science, moderation, and (anti)enthusiasm were conceptually connected. The emergence of political science, understood in this way, required a number of conceptual changes in a structure of argument shaped largely by Locke. These conceptual changes, in turn, fixed a rhetorical framework for persistent debates over the methodological and political identity of political science, even as ideology *literally replaced enthusiasm. These persistent debates reveal the relevance of the history of political science as a forum for remembrance, reflection, and critique.*

Adam Smith plays a very visible hand in the emergence of modern political science. Indeed, historians of political science can find in the *Wealth of Nations* an important set of clues about the intentions and character of political science at its formation. These clues are to be found in those state policies that Smith proposes and justifies in the matter of "the labouring poor, that is, the great body of the people" (1976, 2:302–3). As a moderate Whig adviser in political economy—that "branch of the science of a statesman" concerned to preserve "the system of natural liberty" (1:449, 2:208)—Smith counsels the state to act vigilantly and constantly to prevent the disfiguring of popular morals and political attitudes due to urbanization. In rural environments, he admits, statist remedies are quite unnecessary because the "country village" buoys up the morals of "the man of low condition"; "but as soon as he comes into a great city, he is sunk in obscurity and darkness." Understandably, though regrettably, he may turn to a "small religious sect" for a sense of place and light. In so doing, his morals will be transformed into "rather disgreeably rigorous and unsocial" ones. In the name of moderation, this must be prevented at all costs. Thus, the state should enact at least two sorts of remedies. For one, it should stimulate "the frequency and gaiety of public diversions" so as to "dissipate that melancholy and gloomy humour which is almost always the nurse of popular superstition and enthusiasm." It should also universalize, amongst the middling ranks and above, "the study of science" in order that its good effects may trickle down: "Science is the great antidote to the poison of enthusiasm and superstition" (2:317–18).

By 1776, Smith was not alone in making these sorts of proposals. Nor was he alone in advancing the more general views about science, moderation, and enthusiasm that attended and purported to warrant them. Indeed, he was in the company of a number of his countrymen who were later heralded as the leading

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figures of a Scottish Enlightenment. Among his company, Smith numbered Adam Ferguson and especially David Hume, "by far the most illustrious philosopher and historian of the present age" (2:311). Among other things, Hume, Smith, Ferguson, and their Scottish cohorts were involved in transforming the ideas of John Locke, which were so influential on the Enlightenment as a whole. The emergence of political science is one of the more important consequences of this transformation.

In narrative form, I explore the nature and meaning of this episode in the history of political science.[1] Some of my conclusions may be stated here in advance before we return to the words of the past. The expressly articulated concept of a "science of politics" emerges out of the "moral sciences" in the Scottish Enlightenment in the eighteenth century. It was designed to advance at the same time both a Newtonian *method for the study of politics* and a *politics of moderation* whose tasks included a critique of "enthusiasm" and other excesses of mind and body politic. In this way, the science of politics, the politics of moderation, and the critique of enthusiasm were conceptually connected. The emergence of the science of politics required a number of conceptual changes in a structure of moral and scientific argument whose contours were shaped largely by Locke. These conceptual changes, in turn, fixed a rhetorical framework for future debates over the methodological and political identity of political science in the Anglo-American world. These debates persisted and persist even as *ideology* literally replaces *enthusiasm* as the name of the poison to be remedied by the scientific moderation of political science. These conclusions point to the importance of understanding conceptual change, developing a political philosophy of science, and getting our history right.

The New Science of Politics

There are many candidates for the distinction of being called the first political scientist. To retreat in time, the names of Lasswell, Merriam, Bentley, Lieber, Mill, Hobbes, and Machiavelli have all been singled out as founders of our discipline. A recent president of the American Political Science Association honored an even more ancient figure. In what can best be described as an act of exceeding generosity, Aristotle was heralded as "not only the first political scientist, but, indeed, as the first *bio*political scientist" (Wahlke 1979, 27n).

Two tests are relevant for the historically risky task of determining founding fathers in the otherwise stable activity of writing a history of a discipline or of an intellectual practice like political science. The first proceeds from *our* understanding of political science and examines whether those honorably inducted are indeed read and reread in order to inspire contemporary theory. One could be forgiven for thinking that none of the abovementioned figures really commands the attention this first test requires. The other test concerns not our understanding but *theirs*—what they were doing, what they were saying about what they were doing, and what they were doing in what they were saying. Here it matters very much how the intellectual practices undertaken years if not centuries ago were conceptualized and articulated by their practitioners. This is so because a practice—especially perhaps an intellectual practice—must be identified in large part by the concepts its practitioners held or hold about it; and these concepts must be identified in turn by the very language within which they are articulated. For "the surest sign that a group . . . has entered into the self-conscious possession of a new concept is that a correspondent vocabulary will be developed, a vocabulary which

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can then be used to pick out and discuss the concept with consistency" (Skinner 1978, 2:352).

The Anglo-American vocabulary of "political science" or the "science of politics" emerges as a staple of political discourse in the second half of the eighteenth century, roughly from 1740 to 1790. The principle characters behind these newly articulated efforts were Scotsmen: David Hume, Adam Smith, and Adam Ferguson. These thinkers—and others, too, including William Robertson, John Millar, Lord Kames, and Dugald Stewart (at a somewhat later period)—were subsequently judged so illuminating and so strikingly different in the context of Calvinist Scotland that they were credited with having ushered in a Scottish Enlightenment. The science of politics may be understood, then, as one product of this Enlightenment. And indeed, unlike the term *enlightenment*, the term *political science* was expressly used by the new scientists of politics to characterize their joint enterprise. Hume's own hopes—to "introduce the experimental method into moral subjects" (1978, subtitle) so that, among other things, "politics may be reduced to a science" (1748, 20)—were shared by his Scottish cohorts, however much they differed about details or about how these hopes might best be realized in practice. The sense of novelty and urgency was palpable to them all.

These enlightened Scots wrote a new chapter in what they took to be the history of the progress of the human understanding. The study of politics was, of course, an ancient and venerable enterprise of the human understanding, judging by the Anglo-American tradition alone. Calling theirs a "science" of politics, then, was of no small note. Neither Bacon, nor Hobbes, nor Locke, for example, sustained any effort to create what they were willing to repeatedly denominate as a "science of politics," though of course they used the constituent terms often enough. Mentioning Locke in this context is of particular importance, for "Hume's and Smith's moral thinking was initially much preoccupied with remedying what they saw as the defects of Locke's thought" (Dunn 1983, 121). Less remedially, they carried forward and transformed one aspect of the political intent behind Locke's thought, namely the critique of enthusiasm—a subject to which I turn in the next section. Thus Locke marks an important point of departure from which to understand the conceptual changes involved in the formation of a science of politics.

The Scots shared a number of things with Locke: a recognition of the profound limits of the human understanding; an attempt to prescribe general rules to govern the understanding so that it might perform at its humble best; an acknowledgment that "science" named—and the optics and mechanics of Sir Isaac Newton best revealed—the highest exemplification of those general rules; a hope to advance a "moral science" that would hold implications for a right understanding of politics. But these shared commitments conceal as much as they reveal. In particular, they conceal conceptual changes that allowed the "science of politics" to be counted amongst the "moral sciences," properly so called.

Three changes are particularly salient in this stretch of intellectual history from Locke to the Scottish Enlightenment relating to (1) the strength of the cognitive claims for "science"; (2) the nature and range of "moral" phenomena; and (3) the idealization of Sir Isaac Newton as a methodological exemplar.

For Locke, *science* was an honorific term reserved almost exclusively to describe those achievements of the human understanding that laid cognitive claim to *certainty*. Certainty was identified with knowledge, strictly speaking, and was guaranteed either by intuition or demonstration. Intuition directly perceived the

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truth in any relation of ideas, as for example in logical truths. Demonstration required intervening ideas, as for example in mathematical proofs. Any form of cognition that fell beneath certainty—which was to say, most forms—was characterized as a *probability*. Probability characterized a relation of ideas whose command over our considered belief was mitigated in varying degrees by such things as conflicting observations, the testimony of others, or hypotheses into matters beyond our senses (Farr 1987). In short, probability characterized the sort of understanding about which there could be any doubt whatsoever, and this meant any form of what we would now call *empirical inquiry*. Thus, even though probability consumes "the greatest part of our Concernment" here on "this our Pilgrimage," a science it does not make (Locke 1975, 652). With the distinction between certainty and probability firmly in place, Locke could conclude that logic and mathematics were sciences, whereas mechanics and natural philosophy were not. And no amount of human labor or clever experiment could change the score: "How far soever humane Industry may advance useful and experimental Philosophy in physical Things, *scientifical* will still be out of reach" (p. 556).

Human industry may never transform physics into a science, but in the matter of morality it expends itself on a preordained science. However odd this sounds to modern ears so attuned to a debilitating if honest relativism, Locke believed that "the *Idea* of a supreme Being . . . whose Workmanship we are, and on whom we depend; and the *Idea* of our selves . . . afford such Foundations of our Duty and Rules of Action, as might place *Morality amongst the Sciences capable of Demonstration*" (p. 549). Locke went on to say that "Indifferency and Attention"—the nonenthusiastic attitude of moderation and rationality required for this science—will make out "the measures of right and wrong" to be "as incontestable as those in Mathematicks" (p. 549). Locke dramatically reveals here the theocentrism upon which his epistemology was founded. But even though God has underwritten a moral science, so very much remains to be done by his creatures to specify the content of its rules. Locke knew this painfully well, not only to judge by the epistemological difficulties he acknowledges in the *Essay*, but by his failure to draw up an actual list of demonstratively certain moral duties—despite the persistent proddings and requests from his closest friends, especially William Molyneux (Locke 1976–85, nos. 1838, 2030, 2059). On this score Locke left his readers only a few threadbare examples, like "No Government allows Absolute Liberty" and "Where there is no Property, there is no Injustice" (1975, 549). Had Locke ever chosen to characterize any aspect of his enterprise as a "science of politics," this would have been the time to do so. As a matter of public record, however, this he never did.

Hume, Smith, and their Scottish cohorts in fact did characterize their labors in precisely these terms half a century and more later. This was an important conceptual innovation in itself. But behind it were conceptual changes in the meaning of *science*, and of *moral science* in particular. To measure the profundity of these changes, consider Hume's criticism of one of Locke's threadbare examples just cited: "To convince us of this proposition, that *where there is no property, there can be no injustice*, it is only necessary to define the terms, and explain injustice to be a violation of property. This proposition is, indeed, nothing but a more imperfect definition" (1975, 163). But Locke was guilty not only of a verbal sleight of hand; he had been trying to do the impossible, namely, to demonstrate morality: "The only proper objects of . . . demonstration," Hume went on to say, were "the sciences of quantity and num-

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ber" (p. 163). "Morals" (in Locke's sense of *rules of conduct*) "were not so properly objects of the understanding as of taste and sentiment" (p. 165; cf. 1978, 456–70).

This did not mean that a "moral science" was impossible. Quite the contrary. But its very possibility depended upon abandoning a theocentric epistemology and expanding the "moral" domain capable of "science." Indeed the changes may be succinctly characterized as a move "from applied theology to social analysis" (Dunn 1983). In his own time, Adam Ferguson put the point more metaphorically in his *Principles of Moral and Political Science*: "In a period of many pretensions to science" it is necessary "to recall the attention of mankind from the heavens to the earth" (1792, 1:4). When Hume advertised the *Treatise of Human Nature* by subtitling it as an "attempt to introduce the experimental method into moral subjects," those moral subjects were expanded to include virtually all earthly things social or human, including, in principle, deviations from the rules of right conduct, especially rational Christian conduct. Implicit in this expansion was an increasing sensitivity to the relativity of cultural and moral forms and a certain moderate—or "mitigated"—skepticism that invariably attends such sensitivity. Hume not only said this but along with Smith, Ferguson, Robertson, and Millar showed it in practice by moving beyond theocentrism and its "moral" limitations. They produced comparative studies of forms of government, histories of the progress of civil society, discourses on natural religion, and inquiries into political economy, especially those detailing the nature and causes of the wealth of nations.

These various enterprises in social analysis were factual and causal inquiries that no one scrupled to disallow as "science." Thus the demonstrative sciences like logic and mathematics were now joined by other sciences, including "moral reasonings . . . either concerning particular or general facts." History and astronomy were examples of sciences of particular facts. "The sciences which treat of general facts are politics, natural philosophy, physic, chemistry, etc. where the qualities, causes and effects of a whole species of objects are enquired into" (Hume 1975, 165).

Calling politics a *science*, and ranking it alongside physics, depended upon two further developments. The first was an increasingly evident tendency to liberalize what could in principle pass muster as "science." Hume, along with Smith and others of his generation, promoted into the ranks of science those forms of factual inquiry that laid cognitive claim to *probability*—especially probability in its strong form as *proof*, something about which Hume (wrongly) thought Locke oblivious (1975, 56n). Hume did, of course, place great strictures upon how much confidence we might place in our attributions of probability, whether in matters of evidence or of causality. And he unleashed his most astringent comments on those who believed they could go beyond these strictures, especially those who believed in miracles. For Hume, the only miracle was that anyone could believe in one (p. 131). All of this was a consequence of his mitigated skepticism and his recognition of the profound limits of the human understanding. But probabilistic inquiries were still in principle sciences. Though liberal, this did not signal an attitude of "anything goes" when it came to demarcating a science. In his more inquisitorial moods Hume was willing to "commit . . . to the flames" such cryptocognitive practices as "metaphysics" since it was "not properly a science" (p. 165). Smith was only slightly less ungenerous, facetiously calling metaphysics a "cobweb science" (1976, 2:293).

In practice, of course, the science of politics had considerable distance to travel to justly deserve its place alongside

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physics.[2] Hume's own particular program —inspired in part by Francis Hutcheson (1755) and joined by Smith, Ferguson, and others—to ensure the methodological success of the science of politics depended upon a certain architecture of social theory. "Politics," he averred, "consider men as united in society, and dependent on each other" (1978, xv). Mutual dependence and social unity required sympathy and other moral sentiments about which theories must be developed. Underneath this superstructure Hume laid his foundation in a theory of the human understanding whose hallmark was the association of ideas. This foundational theory had its visionary potential in Hume's eyes: "By this means, we may perhaps, attain a new microscope or species of optics . . . in the moral sciences" (1975, 62).

The very mention of microscopes and optics in the moral sciences intimates the other notable development that made possible Hume's juxtaposition of politics and physics: namely, the incredible power of the image of a moral science modeled on Sir Isaac Newton's physics. Following Newton's lead, Hume envisioned (at least in his more prophetic and positivistic moods) an experimental and law-governed methodology for science. It was to be one in which, as Ferguson put it, "the Regulae Philosophica [of Newton's *Principia*] . . . must be sustained" (1792, 2:118). The Scots had even greater hopes to mobilize Newton's mechanical principles in their own substantive theories. "ATTRACTION," Hume shouted in bold letters, characterized a principle of the association of ideas and of sympathy (1978, 12–13). Action and reaction was "a maxim of the science of mechanics" good for the moral and political sciences, Ferguson noted (1792, 1:11). The imaginative fixation on Newton was so compelling that Hume hoped to be named the Newton of the moral sciences. John Millar later complicated that particular hope, but underscored its power. He paid that tribute to the author of the *Wealth of Nations* (quoted in Smith 1980, 275n).

Here, too, is the third major change that informs our narrative. "The incomparable Mr. Newton" was of course well known to Locke. Among other things, Locke modestly hoped to be his "Underlabourer" (1975, 10). But Locke marveled at the complexity of Newton, a complexity all but lost on Hume, Smith, and countless generations since. Locke was privy to Newton's enormous range of interests not only in natural philosophy (which Locke understood only in part) but in religious hypotheses, biblical prophecy, numerology, and other "mystical fansies," which Newton shared with him in correspondence (Locke 1976–85, no. 1357). Among Locke's contemporaries, the latitudinarian clergy promoted their gospel of tolerant Christian rationalism partly by appealing to Newton's own vision of the harmony of "the world natural and the world politic" (quoted in Jacobs 1976, preface). In short, the imaginative possibilities that Newton symbolized for numerous intellectuals and clergymen at the turn of the eighteenth century had changed dramatically by the time of the Scottish Enlightenment. Newton had become a uniquely methodological and scientific figure, though one of imperial proportions (Smith 1980, 104). The moral sciences were potential colonies, and could only take their place in the sun by following the lead of Newtonian mechanics. It was this view that Dugald Stewart was to call "the peculiar glory of the latter half of the eighteenth century" (1854, 1:70).

In sum, by the time the Scots furthered the "moral sciences" and expressly individuated a "science of politics" out of them, the conceptual materials that Locke structured had been radically transformed. No longer need the moral sciences bear the burden Locke placed upon them. No longer, that is, need they pro-

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vide a demonstratively certain understanding of the prescriptive moral duties binding the consciences and conduct of rational Christians. Rather, the moral sciences could move forward as probabilistic forms of empirical and causal inquiry into social or human matters that took methodological inspiration from the natural sciences, especially Newtonian mechanics. Here at the veritable coining of *political science* we recognize a glimmer of the modern conception of the nature and limits of what have come to be called the social, human, or behavioral sciences, even though an endless series of debates were to govern the constitution, specification, and transformation of these sciences, even to the present time.

An Antidote to Enthusiasm

Setting methodological standards did not constitute an insular academic exercise for the Scottish founders of the science of politics. Indeed, in and through setting methodological standards, the Scots were at the same time thinking and acting politically. Their science was to be an instrument of political education and criticism in the interests of moderation. "Nothing," Smith declared, "tends so much to promote public spirit as the study of politics" (1969, 307). Hume agreed: "Those who employ their pens on political subjects, free from Party-Rage and Party-Prejudices, cultivate a science, which, of all others, contributes most . . . to public utility" (1748, 127). The accent on *rage* and *prejudice* put some teeth into these otherwise innocuous-sounding gestures of public-mindedness. Such excessively immoderate expressions as rage and prejudice were symptoms of political passions and attitudes that in an era of faction, disorder, and rebellion were best likened, as Smith likened them, to "poisons."

This heightened and even hyperbolic language suggests the stakes for the Scots, as well as their fervor for science. These days we might be tempted to call it an enthusiasm for political science. But this is precisely what our eighteenth century Scottish Enlighteners could not have called it. In their terms, "enthusiasm"—as well as "superstition"—posed two of the greatest threats to the mind and the body politic. The Scots found enthusiasm and superstition to be such virulent strains of poison largely because they diagnosed them so broadly as to encompass all manner of immoderation in religion and politics—especially but not invariably amongst the lower orders. Enthusiastic and superstitious claims about the human understanding—about what could be known or justifiably believed, especially in matters informing action and conduct—lay at the foundation of this immoderation. This naturally drew the attention and the ire of the Scottish scientists of politics. The full story of the political formation of the science of politics requires equal attention to both of these poisons—as well as to yet other political intentions and projects. But this cannot be done here, and it is certainly the case that superstition is a relatively well worked theme in studies of Hume, Smith, and the Enlightenment generally (Gay 1966). Perhaps, then, we might mention it only briefly here, so as to press on to its understudied twin.

Superstition—on the telling of its countless enemies, at least—manifested itself in any number of irrationalities harbored by the understanding. But it especially emerged in "fear of harm and disorder from invisible powers" (Robertson 1777, 1:305), which, as Hume noted, became institutionalized in "Ceremonies, Observances, Mortifications, Sacrifices, Presents, or in any Practice, however absurd or frivolous, which either Folly or Knavery recommends to a blind and terrified Credulity" (1748, 106). Although ancient pagans, Indians, Highlanders,

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high Anglicans, and Jews were not exempt from its charms, "the grossest delusions of superstition" (Smith 1976, 2:325) particularly infected Roman Catholicism. "Priestcraft" and "priestly power" exercised an authoritarian hold over the minds, if not the souls, of benighted papists. The critique of superstition in this its Romish guise had long figured in the polemical battles of the English and Scottish Reformations. The Scottish Enlighteners carried on in this task. In the *Principles of Moral and Political Science*, Ferguson epistemologically indicted the superstition of priests and papes for having "set reason aside" and so become "an engine to work up the minds of other men to their purpose" (1792, 1:147). In this he agreed with Hume who had already drawn a political conclusion about this engine. Superstition was "an Enemy to Civil Liberty," for it "renders Men tame, and abject, and fits them for Slavery" (1748, 108, 111).

Enthusiasm threatened the minds of men in different but equally poisonous ways. The Scots' diagnosis of it was not utterly unique in the Anglo-American tradition. To appreciate both continuity and change we may briefly trace its history, a history told largely by its critics. The idea of enthusiasm entered the English language in the late sixteenth century, bringing with it the Greek notion of divine inspiration, of "God possessing Man." Very shortly in Reformation Britain, however, it took on the decidedly more suspicious connotation of *delusions* of inspiration or of *false* claims to having certain and immediate knowledge of God. In this guise, it generally became a convenient epithet of abuse employed by countless writers who sought to discredit the zealotry of the fringe sects thrown up by the Reformation and the English Revolution of the 1640s. Quakers, Baptists, Antinomians, Diggers, Ranters, Familists, Fifth Monarchists, and other sectaries were excoriated for their beliefs and for their appeals to (an alleged) divine inspiration as the source of these beliefs. Hobbes, predictably, defined as "madmen Prophets" those who "pretended to Prophecy by possession of a Spirit." He heard in "such Enthusiasme" another loud note amidst "the Seditious roaring of a troubled Nation" (1968, 141, 143). Treatises poured out, under titles that invoked the specter of *Enthusiasticus Triumphatus* or that, conversely, announced with hope of relief *The Spirit of Enthusiasm Exorcised*. By the late seventeenth century, the Anglican clergy could weigh up all the enthusiastic sects as "vast Loads of Filth" (quoted in Jacobs 1976, 193).

Locke, too, lodged warnings and complaints against "the Extravagancy of Enthusiasm" (1975, 667), although he was neither Hobbist nor friend to the Anglican clergy (Goldie 1983). Indeed, the topic was of such importance to him that he added a wholly new chapter, "Of Enthusiasm," to the fourth edition of the *Essay Concerning Human Understanding*. His analysis of it was tied to his theocentrism, as well as to questions of "method" —a point he makes clear in the juxtaposition of entries in his commonplace books (see Farr 1987, appendix). Locke proceeds to explain "the rise" of enthusiasm in the individual mind, to undermine the force of its claims, and to reaffirm the role of reason in the human and Christian understanding.

"Men, in whom Melancholy has mixed with devotion," Locke explains, readily tire of "the tedious and not always successful Labour of strict Reasoning." They are thus "very apt to pretend to Revelation": "Their Minds being thus prepared, whatever groundless Opinion comes to settle it self strongly upon their Fancies, is an Illumination from the Spirit of GOD, and presently of divine Authority: And whatsoever odd Action they find in themselves a Strong Inclination to do, that impulse is concluded to be a call or direction from Heaven, and must be obeyed;

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'tis a Commission from above, and they cannot err in executing it" (1975, 699). Using his own very strict language of science, Locke castigates this as an unwarranted claim to "Certainty without proof, and without examination" (p. 700). It confuses genuine understanding with seeing and feeling, especially because of the oft-abused simile of "a light within." It also substitutes the immediacy and strength of belief for rational argument or scriptural evidence. Locke's training as a physician allowed him to find not revelation in all this enthusiastic exultation of "certainty in themselves, and demonstration to others" but only the "Conceits of a warmed or over-weening Brain," which fuel the "untractable Zealots in different and opposite Parties" (pp. 699–700, 703). After having identified enthusiasm in this way, Locke begged off writing "an historical account of the various ravings [that] men have embraced for religion" because it was "besides my purpose, and enough to make a huge volume" (1976–85, no. 1887).

Doubtless, Locke felt with considerable discomfort the challenge that enthusiasm posed; for whatever its extravagance, enthusiasm still held out something of Locke's own hope that moral knowledge could be shown to be certain knowledge. Moreover, Locke could not and never tried to disallow revelation and miracles as grounds for moral belief: "I am far from denying, that GOD can, or doth sometimes enlighten Mens Minds in the apprehending certain Truths, or excite them to Good Actions by the immediate influence and assistance of the Holy Spirit" (1975, 705). Locke settled, as he had to, for hedging round revelation and miracles with reason and Scripture read aright. And he had to settle for arguing negatively against enthusiasm because, as mentioned above, he failed in the constructive task of demonstrating morality and so showing it to be, on his own terms, "scientific." For these reasons, perhaps, Locke never clearly tells his readers what to *do* about enthusiasm (though he was ready to put priests to a "test" and atheists to much worse). On pain of intoleration, it would appear that Locke himself in the *Essay* sets the example for the only acceptable action against enthusiasm: arguing rationally against it.

The Scottish scientists of politics had a slightly better formed idea of what must be done to combat enthusiasm publicly. But this was itself a consequence of changes in their understanding of science and in their diagnosis of the *social mechanisms* by which enthusiasm was propagated. Here the Scots followed the lead of Anthony Ashley Cooper, the third earl of Shaftesbury, a boyhood pupil of Locke's and the grandson of Locke's radical Whig patron and employer. (Locke's own radicalism, only now being fully documented [Ashcraft 1986], might be credited in good measure to the *first* earl's influence.)

In *A Letter Concerning Enthusiasm*, occasioned by the arrival in London of the so-called French Prophets, Shaftesbury espied the same "Specious Pretext of moral Certainty" (1727, 1:44) which, alongside an alleged divine inspiration, defined enthusiasm. Yet Shaftesbury pressed on in two new directions. One was to allow for an occasional "noble enthusiasm" whose appeals were different from but consistent with the demands of reason (1:54). The other was to emphasize the spread of (the more common, vulgar forms of) enthusiasm, using the sociomedical vocabulary of *effluvia, transpiration, contagion, infection, fury*, and *panic* (see Schwartz 1978, 41–56). The general causal mechanism of "sympathy" was at work here, a mechanism later to become a hallmark of Scottish philosophy after Hutcheson (1755). In the hands of Hume (1978, bk. 2) and Smith (1969), sympathy became the principal form of communication that made society possible. But on occasion it endangered it, too. Shaftesbury explained that "one may with good

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reason call every Passion Panick which is rais'd in a Multitude, and convey'd by . . . Contact or Sympathy. . . . And thus is Religion also Panick, when Enthusiasm of any Kind gets up" (1727, 1:15–16). The magistrate must combat this enthusiastic panic not with violence but with "chearful means" and a more impartial sympathy: "The only way to save Mens Sense, or preserve Wit at all in the World, is to give Liberty to Wit" (1:17, 19). And so Shaftesbury concludes his *Letter* with words that in all likelihood inspired Adam Smith's line a half century later: "By this means we prepare ourselves with some Antidote against Enthusiasm. And this is what I have dar'd affirm is best perform'd by keeping to GOOD HUMOUR. For otherwise the Remedy itself may turn to the Disease" (Shaftesbury 1727, 1:54–55).

Shaftesbury's cure may have seemed a trifle lighthearted, given the disease. But in rough outline the Scots shared his general diagnosis and developed it in accord with their own post-Lockean views of philosophy and science. Enthusiasm was identified by its unscientific extravagance: its boasts of a certainty that "shuts up the mind against the conviction of errour" (Ferguson 1792, 1:145); its failure to respect facts, natural causes, and the bounds of probability in matters moral; its unwillingness to correct its particular judgments in accordance with general rules (Hume 1978, 631); and its conviction of divine inspiration—"the Summit of Enthusiasm" (Hume 1748, 106). Furthermore, as Ferguson noted in his *Political Science*, enthusiasm spread like the "contagion" of a social disease (1792, 1:143), especially amongst the uneducated and witless general public. Like cowardice, which, too, affected the health of mind and body politic, enthusiasm scarcely seemed different from "leprosy or any other loathsome and offensive disease" (Smith 1976, 2:308).

The Scots went beyond Shaftesbury's diagnosis to discover and describe all manner of enthusiasm in history. The contrast with the general health and progress of modern commercial societies was thereby made all the more dramatic. Ferguson found it amongst ancient Greek republicans and Indians of the subcontinent (1767, 127; 1792, 2:141); Robertson found it amongst early Highlander nationalists and Indians of America (1777, 1:390; 1844, 11); Smith found it amongst the Independents during the English Revolution and "the dissenters and the methodists" of his own day (1976, 2:310). Hume found it virtually everywhere, amongst "the Anabaptists in Germany, the Camisars in France, the Levelers and other Fanaticks in England, and the Covenanters in Scotland" (1748, 110; also see his *History of England* [1754], especially the infamous volume on the Stuarts and the Commonwealth). No noble enthusiasm inspired these various sectaries. However, in Shaftesbury's spirit, Hume did allow that in political matters enthusiasm was "naturally accompanied by a Spirit of Liberty" (1748, 111)—a point that influenced Gibbon in his otherwise critical account of ancient enthusiasm (Pocock 1985, chap. 8). Against the authoritarian and enslaving doctrines of superstition, this was to enthusiasm's credit. However, liberty was fraught with perils when too enthused. Thus from the history of "dangerous enthusiasts," Hume confirmed the recurring voices for "leveling" and "a community of goods" (1975, 186, 193); Smith, "the interested complaints of faction and sedition" (1976, 2:309); and Ferguson, the contagious communications of "revolutionists in government" (1792, 1:135).

For fear of political excesses such as these, government had to actively oppose enthusiasm. Science was a partisan in this opposition. Scientists and philosophers were themselves trustworthy because "there is no enthusiasm among philosophers" (Hume 1975, 147). "Nothing but philosophy," Hume went on in his essay,

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"Of Enthusiasm and Superstition," is "able to conquer entirely these unaccountable Terrors" (1748, 108). The engagement implied by these words suggest that philosophy and science represented not only enthusiasm's *opposite* but—in Smith's striking phrase—its *antidote*. The matter may be put somewhat differently by developing Ferguson's observation that "the affairs of society require the light of science" (1792, 1:269). Not only was science to shed light *on* the affairs of society, as it were; it was—to lift a line from the enthusiasts themselves—to shine like a light *within* society.

Adam Smith was the most forthcoming in presenting a basic strategy for the state's campaign against enthusiasm, though concurring sentiments can be found amongst his countrymen. As was his wont, our "professor of moral philosophy" did not construct a *heads of proposals*, much less a *manifesto*. But he made his views sufficiently clear in the *Wealth of Nations*, especially in the articles on education and religious instruction in book 4, part 3, "Of the Expence of public Works and public Institutions." State violence was to play no part. Neither would one state church nor would many legal edicts. Rather, the first plank of his program was religious toleration. Here he followed Locke and Shaftesbury and a whole host of eighteenth century tolerationists. But Smith's reasons included something of a hidden-hand argument above and beyond tolerationist considerations respecting doctrinal differences or individual moral development: "Where the society is divided into two or three hundred, or perhaps as many thousand small sects . . . no one could be considerable enough to disturb the public tranquility. The teachers of each sect, seeing themselves surrounded on all sides with more adversaries than friends, would be obliged to learn . . . candour and moderation" (1976, 2:314).

For obvious reasons, however, a policy of toleration was in itself insufficient to prevent the public disorders attendant to enthusiasm. More direct action by way of education—that enlightenment gamble—was required by the state. First came direct general education of the young, especially those of the laboring poor. Their virtues and understandings were constantly threatened by the division of labor and other unavoidable corruptions of a commercial society. The stability of government was palpably at stake.

> The State derives no inconsiderable advantage from their instruction. The more thay are instructed, the less liable they are to the delusions of enthusiasm and superstition. . . . An instructed and intelligent people besides, are always more decent and orderly than an ignorant and stupid one. . . . They are more disposed to examine, and more capable of seeing through, the interested complaints of faction and sedition, and they are, upon that account, less apt to be misled into any wanton or unnecessary opposition to the measures of government. (2:309)

Beyond general education, Smith counseled further "Institutions for the Instruction of People of all Ages," the chief of which was to be for "religious instruction" in the broadest sense imaginable (2:309). After lengthy analysis and a harangue against "stupid and ignorant enthusiasts," Smith advises the "wise legislator" in the two principal remedies that were noted at the outset of this essay. One, we may recall, provided for "public diversions" in the form of "dramatic representations and exhibitions" in order to dissipate gloom and melancholy. This rendered in a more publicly spectatorial way Shaftesbury's design for "gaiety and good humour . . . and public ridicule," as Smith himself put it (2:318). The circuses of the ancient world had their place in modern commercial society, after all.

The other remedy provided for "the study of science and philosophy, which the state might render almost universal among all people of middling, or more than middling rank and fortune." Here the state could take the low road of insist-

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ing only upon certain standards that a candidate for "any liberal profession . . . or any honourable office" might meet. This would force them, marketwise, to find better teachers than the state could provide directly. But the end gave purpose to these means; for, however achieved, a scientific education was necessary for the health and order of the people: "Science is the great antidote to the poison of enthusiasm and superstition, and where all the superiour rank of people were secured from it, the inferior ranks could not be much exposed to it" (2:318).

Protected from the poisons of enthusiasm and superstition, both mind and body politic may then attempt to govern themselves as an "impartial spectator" might. Impartiality of the spectatorial sort required "good temper and moderation" (2:297, 315). Thus, in advancing the cause of a science of politics, the figures of the Scottish Enlightenment *at the same time* furthered the cause of a politics of moderation and its attendant virtues—impartiality, candor, and coolness of temper. This was not, in their eyes, simply a matter of choice. Rather, it was *required* by the structure of their political argument, which conceptually connected philosophy, science, moderation, and the absence of enthusiasm. Thus, when Hume opened his famous essay, "That Politics May Be Reduced to a Science," he presented himself as he felt he had to—as a "friend of *Moderation*" (1748, 20).

Moderation secured for its Scottish friends a political position with considerable rhetorical opportunities, to say the least. It characterized a calm and detached mode of political argument more than it picked out a particular agenda. Among moderation's rhetorical virtues, it allowed the Scots to be—or at least to claim to be—both detached and committed, both scientific and political. It also earned them the subsequent charge of conservatism. Given the animus that Hume showed towards Levelers or that Smith showed towards Dissenters, the charge is not utterly undeserved. A more generous reading is possible: "In its full extent and in its historical circumstances, Hume's science of politics was constructive, forward-looking, a programme of modernization, an education for backward-looking men" (Forbes 1977, 39). In any case, if *conservative* aptly describes the "Lesson of Moderation" that Hume and Smith and Ferguson drew time and time again, then surely it is "the result of an analysis derived from a philosophical position rather than a mere attitude of conservatism" (Winch 1978, 171). The lesson of moderation *had* to be drawn from the very nature of the science of politics.

Ideology and the History of Political Science

Curiosity naturally presses a narrative beyond the historical limits it sets itself. One wants to know what, if anything, came of the past? What, in this case, came of the Scottish Enlighteners' expressly articulated intention to create a "science of politics" that would advance both a natural scientific methodology whose inspiration was Newtonian and a politics of moderation whose tasks included the critique of enthusiasm? This is a difficult question to answer, in part because of the sheer mass of historical materials needed to satisfy curiosity and in part because of the evident fact that so very much has changed. The Scottish Enlightenment does not immediately spring to mind when contemporary political scientists set themselves to solve theoretical, methodological, or political problems of current concern. Or, to use Marx's haunting metaphor, the Scottish Enlightenment does not, in political scientists' waking—or sleeping—hours, "weigh like a nightmare on the brain of the living" (1963, 15) (though I venture to predict some night-

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mares now that political scientists are bringing political economy and the state back in). Let me conclude this essay with some further historical and theoretical reflections that might begin to alleviate the curiosity behind the above question and to do so fully cognizant of change. I hope, thereby, to intimate the ongoing relevance of the history of political science for our disciplinary self-conceptions.

The call for a science of politics newly modeled on the natural sciences in Newtonian and probabilistic form continues to be heard well after Dugald Stewart praised this "peculiar glory" of Hume, Smith, and Ferguson. One hears it in Comte, Mill, Spencer, Bryce, Merriam, Lasswell, Easton, and not a few behavioral revolutionaries. So time honored has this call become that, as far as the history of political science is concerned, one need only highlight the faint irony that attends it. One of the oldest things about political science is its methodological promise to be new—and Newtonian! Though this view has met more than one critic, it has generally set the terms of debates over the *methodological* identity of political science. And so the echoes of the Scottish Enlightenment can be heard in our own debates today.

The science of politics continues to be pressed into political debates, as well. For a time, these political debates continued to be rhetorically heightened by the charges of enthusiasm. These charges wane soon enough, however, as the meaning of *enthusiasm* changes, and one ideology or another—indeed, *ideology* as such—comes to replace it. But the *political* identity of a science of politics committed to moderation lingers on well after "enthusiasm" receives the rather unintended antidote of conceptual change.

Consider one episode of interest and importance to students of U.S. politics: the debates in 1787–88 over the yet-unratified Constitution. The mock Romans on both sides—Cato, Brutus, Publius—claim to speak on the authority of the "science of politics," or the related sciences of government, legislation, and statesmanship (for anti-Federalists, see Storing 1981, 2.7.3 on the juxtaposition of "the science of government" with "that frenzy of enthusiasm"; for Federalists, see Wood 1969, chaps. 1 and 15). In the case of Publius (Hamilton, Madison, and Jay 1961), the Scottish connection is strikingly evident (Adair 1957; Wills 1981).

In defending the Constitution's plan for an extended compound federal republic, Publius opens the *Federalist* with a "lesson of moderation"; and closes it by citing Hume's essay, "Of the Rise and Progress of Arts and Sciences." "These reflections contain a lesson of moderation to all the sincere lovers of the Union" (Hamilton, Madison, and Jay 1961, 34, 514). In the course of the 85 papers—most evidently in numbers 9, 31, 37, and 47—the "science of politics" takes a stand as a methodological and political ally of the Federalist rhetoric of moderation and union. Eschewing pretences to "the same degree of certainty with those of the mathematics," the science of politics nonetheless has "better claims" than ordinary opinion (p. 194). Thus it counsels "caution and investigation" as "necessary armor against error and imposition" (p. 194). Without this armor a nation comes to suffer "moral diseases," "poisons," and "the infection of violent passions" (pp. 99, 147, 385). Such maladies attend a zealous and factious politics of the sort Publius fears is everywhere evident in the state governments under the Articles of Confederation.

But the science of politics understands the republican mechanisms necessary to alleviate these ills. Consider the hidden hand of national size at work against a familiar source of trouble: "A religious sect may degenerate into a political faction in a part of the Confederacy; but the variety of sects dispersed over the entire

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face of it [the nation] must secure the national councils against any danger from that source" (p. 84). (The comparisons with Smith are too striking to miss; see Smith 1976, 2:314.) So it is that the science of politics plays its rather more visible hand for "the necessity of Union," since union alone is "the proper antidote for the diseases of faction" (Hamilton, Madison, and Jay 1961, 99).

Given the Scottish intellectual sources and, even more, given the history of religious enthusiasm in the colonies (Lovejoy 1985), it is not surprising that *enthusiasm* makes its appearance in these debates. In the very first paper—and with strong Shaftesburian and Humean overtones—Hamilton warns his readers against those zealots who in the name of liberty cry out against union. For such "noble enthusiasm of liberty is apt to be infected with a spirit of narrow and illiberal distrust" (Hamilton, Madison, and Jay 1961, 35). Later, in cataloging the problems created by the bevy of state governments under the Articles, Madison reminds his postrevolutionary readers that such governments were formed in the midst of "an enthusiastic confidence of the people in their patriotic leaders, which stifled the ordinary diversity of opinion in great national questions" (p. 315).[3] This echoed Madison's prerevolutionary sentiments, in which he contrasted "enthusiasm" with "sober public opinion" (quoted in Lovejoy 1985, 220). But in great national questions there should be sobriety and diversity of opinion. And surely, one "ought not to assume an infallibility" (Hamilton, Madison, and Jay 1961, 226), for none is to be had, not even from God: "When the Almighty himself condescends to address mankind in their own language, his meaning, luminous as it must be, is rendered dim and doubtful by the cloudy medium through which it is communicated" (p. 229). Science, in short, represents the best citizens can hope for by way of understanding politics and constituting a new polity (Ball 1983). It serves the interests not of the enthusiastic but only of the "enlightened friends of liberty" (Hamilton, Madison, and Jay 1961, 72).

Social, political, and conceptual processes quickened the pace of change inaugurated by the great revolutions in America and France. Reflecting increasing patterns of secularization and recording an attempt to resolve the contradiction between moderation and the practical requirements of boldly meeting novel problems, *enthusiasm* started to lose much of its sectarian identification and its connotation of divine inspiration. It even came to have its champions. The twice revolutionary Tom Paine, who had spoken for *Common Sense* in the American Revolution, reminded the French Convention in July 1795 that it was not "cold-indifference" but "the enthusiasm and energy which have hitherto been the life and soul of the Revolution" (quoted in Tucker 1972, 100).

The French Revolution continued to divide opinion well after the blades of the law quit falling. "Enthusiasm" and the "science of politics" were parties to these divisions. Macaulay's encomium to "that noble Science of Politics" was contrasted to "the barren theories of utilitarian sophists," whose political commitments, he thought, branded them as a "Republican sect . . . enthusiastically attached to its ends . . . as the French Jacobins themselves" (quoted in Collini, Winch, and Burrow 1983, 101). The Scottish connection was again in evidence, though by now Hume's legacy seemed to bifurcate: historical inductivism for Macaulay's "noble science," utilitarian deductivism for James Mill's "science of government." John Stuart Mill later reflected on this methodological and political episode with some pain, trying in his own way to walk a via media between Macaulay and his father, whom he immortalized as "the last survivor of this great school" of Scottish

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Enlightenment philosophers (1963, 566). The younger Mill helped advance the cause of the "moral sciences," especially in book 6 of his *Logic*. In addition to others, Comte had been instrumental in forming Mill's scientific views. Without hint of disgrace, Mill remembered Comte as having "rekindled all my enthusiasm" (1924, 147), even though Mill had by then disassociated himself from the religious extravagance (not to say *enthusiasm*) of Comte's positivism.

Mill's ability to use *enthusiasm* in this way, and without opposition to *moral science*, marks considerable conceptual change. In spite of occasional archaic uses and reminders, *enthusiasm* was by then assuming its contemporary meaning. The term now denotes intense interest, excitement, or eagerness in any sort of pursuit. It has certainly lost its sectarian reference, pejorative connotation, and opposition to science. Little trace of those seventeenth and eighteenth century debates can be hard these days, what with references in scientific contexts to "the enthusiasm for developing the orthodox position" (Kitcher 1982, 119). With some historical irony, we even hear from two of his excellent editors and commentators that "[Adam] Smith wrote of the Newtonian system with real enthusiasm" (Skinner and Raphael quoted in Smith 1980, 32).

Ideology replaces enthusiasm, literally and functionally. That is, the concept of *ideology* is the direct descendant of *enthusiasm*—as well as of the other ancestral extravagances found in "superstition" and "priestcraft" (see Goldie 1988). *Ideology* takes the place of *enthusiasm* as the opposite of science and the standing danger to reasonableness and moderation in mind and body politic. The history of *ideology* is not without its own fairly dramatic conceptual changes, but the moral remains much the same (Goldie 1988, Lichtheim 1967). *Ideology* was coined by Destutt de Tracy in the post-revolutionary France of 1796. By it, he meant simply the "science of ideas," in Locke's sense of *ideas* and Newton's sense of *science*. This science conceived of its political task as one of enlightenment, rather as did the science of politics to the Scots. It was to liberate human ideas from illusion, predicated on the diagnosis of human ignorance, dogma, and prejudice. But by the late 1840s—thanks practically to Napoleon's expressed disdain for *les ideologues* and theoretically to Marx's no less disdainful criticism of Feuerbach and the Young Hegelians—*ideology* changed its meaning from a science against illusion to illusion itself. Religion—and for Marx all religion suffers from enthusiasm, superstition, and worse—proves to be the veritable exemplar of those illusions. Marx's "method of approach," so he argues, sharply contrasts "religion, metaphysics, [and] all the rest of ideology" with "real, positive science" (Marx and Engels 1970, 47–48).

On this score, at least, twentieth century political scientists have largely agreed with Marx—even if, to many, Marxism itself has become virtually synonymous with ideology. While there are surely some neutral uses, the sense of *ideology* that has been of greatest interest and dispute has referred generally to any systematic set of ideas that is methodologically impervious to rational and empirical inquiry and is politically extremist in the face of moderate alternatives. Ideology, in short, is the antinomy of science both in matters of method and politics. And science not only identifies itself as the opposite of ideology, it promises an end of ideology. Thereby, the scientific antidote to enthusiasm retains its potency even when the poison becomes ideology.

Saying so doesn't make it so, of course; and political science can be, and has been, criticized for failing to live up to its political promises, whether in the matter of moderation, democracy, or progressive reform (Euben 1970; Ricci 1984; Seidel-

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man and Harpham 1985). It has even been criticized for having capitulated to an ideology of the end-of-ideology (MacIntyre 1971, chap. 1). But this only underscores the more fundamental point. The identity of political science is as much a political as a methodological one; and these days this is rhetorically bound up with debates implicating ideology, as earlier it was in debates implicating enthusiasm and superstition. One does not, therefore, have to strain to make the general case that there are ideological implications of political science. The history of political science quite literally records them.

Many of the debates over the ideological implications of political science *in fact* have been occasioned by published histories of political science. This suggests just how relevant such histories can be to the identity of political science, and indeed even to political discourse in the public arena. Episodes from the history of U.S. political science as recounted in the narratives of Beard (1927), Merriam (1931), Easton (1953), Crick (1959), Storing and associates (1962), Ricci (1984), and Seidelman and Harpham (1985) make this abundantly—and sometimes heatedly—obvious. The history of political science, in short, provides a forum for remembrance, reflection, and critique that contributes as directly as one could imagine to our disciplinary identity, our educational practices, and occasionally our public posture. (For history, political science, and education, see Jacobson 1963 and Sabia 1984.) We try to understand ourselves in light of our history, and often in the process we change our conceptions of our history and of our current condition and future prospects.

Debates over Locke and Smith—to choose from the larger cast with which we began—may appear to be different from, say, debates over Beard, Bentley, Merriam, or Lasswell by their temporal remove, if nothing else. But these thinkers are not intellectual antiques. They even enliven public debate in the United States at present. Apart from numberless political scientists, a chief justice of the Supreme Court, an attorney general, several senators, the head of the Office of Management and Budget, and two Nobel economists have appropriated or castigated the doctrines of Locke and Smith in public in recent times. Debates over property rights, international tariffs, legal reform, and even postal delivery have alluded to Locke and Smith—or, at least, to what some say Locke and Smith said and how it is or is not applicable to our current problems. In a politically charged environment like this one, very much depends upon the rectitude of the interpretation of the meaning of these doctrines. *And so back we go, the better to understand ourselves now and in the future.* I have already given more than a few hints how I think we should understand Locke and Smith, above and beyond their contribution to the emergence of political science and the establishment of a rhetorical framework for subsequent debates about its state and discipline. Locke can be better understood as a theocentric radical than as a liberal. Smith can be better understood as a skeptical statist than as a free-marketeer. These interpretations are admittedly less comforting and self-congratulating than are the usual ones we hear these days. They may or may not be able to bear the ideological load some have placed upon them. But they are no less relevant for that.

Notes

An early version of this essay was written and read at the Summer Institute for the History of Social Scientific Inquiry, held at the Center for Advanced Study in the Behavioral Sciences in 1986. I am indebted to the members of the Institute for discussions about the history of political science, and to the Center for providing a stimulating

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environment. Subsequent versions of the essay benefitted from conversations with Leon Epstein, Peter Euben, Terence Ball, and especially Mary G. Dietz. Thanks are due to them all; of course, they are not responsible for the errors that remain.

1. In an essay of this size and scope, much must be assumed or neglected. Innumerable nuances must be passed over in silence, and otherwise important contrasts between thinkers must be suppressed for the sake of the broader historical sketch. Thankfully, however, much of that historical sketch has been narrated or instructively debated by scholars of Locke (Dunn 1969, 1983; Farr 1987; Goldie 1983; Leary 1980; Yolton 1970), of Hume (Farr 1978, 1982; Forbes 1975, 1977; Livingston 1984; Moore 1977; Norton 1982; Whelan 1985), of Smith (Flew 1985; Haakonsen 1981; Harpham 1984; Skinner 1979; Skinner and Wilson 1975; Winch 1978); of enthusiasm (Knox 1950; Lovejoy 1985; Tucker 1972), of other figures in the Scottish Enlightenment in general (Camic 1983; Hont and Ignatieff 1983; Kettler 1965, 1977; Pocock 1975, chap. 14), and of yet other figures of decided importance for this stretch of intellectual history, especially Montesquieu (Richter 1977). This essay also aims to contribute to the resurgence of interest in the history of political science as an intellectual and political practice (see Collini, Winch, and Burrow 1983; Ricci 1984; Seidelman and Harpham 1985).

2. There are a number of reasons why, on Hume's extended account of "moral science," it could never succeed. For some nonpositivistic readings of Hume, see Farr 1978, 1982 and Livingston 1984.

3. Compare the sentiments Madison expressed to Jefferson in a letter of 24 October 1787 (his general subject is how religion does not constrain interest): "When indeed Religion is kindled into enthusiasm, its force like that of other passions is increased by the sympathy of a multitude. . . . Even in its coolest state, it has been much oftener a motive to oppression than a restraint from it" (quoted in Kammen 1986, 72).

4. Mill may have been influenced by the romantics—Coleridge in particular. For his part, when putting in a good word for enthusiasm, Coleridge casts a glance back at Hume: "Histories incomparably more authentic than Mr. Hume's (nay, spite of himself, even his own history) confirm by irrefragable evidence the aphorism of ancient wisdom that nothing was ever achieved without enthusiasm" (quoted in Tucker 1972, 48).

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James Farr is Associate Professor of Political Science, University of Wisconsin, Madison, WI 53706.

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AMERICAN POLITICAL SCIENCE, LIBERALISM, AND THE INVENTION OF POLITICAL THEORY

JOHN G. GUNNELL
State University of New York
Albany

The contemporary estrangement of political theory from political science is in large measure the product of a quarrel that originated in the challenge to the values of U.S. political science initiated by émigré scholars during the 1940s. The behavioral revolution was in an important respect a conservative rebellion in defense of the values of liberalism and related notions of science, relativism, and historical progress that had traditionally informed the discipline. This controversy in the context of political science fundamentally structured the discourse of academic political theory and the contemporary constitution of the field both as a division of political science and as a wider interdisciplinary enterprise.

Only that which has no history is defineable.

—Nietzsche

This essay, most narrowly specified, is part of an account of the discoursive history of academic political theory in the United States. It can, however, for at least two reasons, be construed as a study in the history of political science. First, the period under consideration, 1940–50, was a crucial one in the development of this discipline, and the issues that arose in the subfield of political theory were determinative with respect to its subsequent evolution. Second, the discourse of political theory was also the basic vehicle for reflection on the state of the discipline, its past, and its future prospects. But the relationship between political science and political theory has, since the beginning of the period in question, been an uneasy one. The principal purpose of this essay is to explain that relationship and to explore the origins of a controversy that fundamentally shaped the structure and content of contemporary academic political theory.

Political theory today has little to say to or about its parent field, and much of political theory is of marginal interest and intelligibility to many political scientists. This estrangement cannot be explained merely in terms of normal trends in professional differentiation. It is in part the legacy of an old quarrel that was one of the principal factors in the emergence of the independent interdisciplinary field of political theory. Since the early 1970s, the subfield of political theory in political science, once understood to be the core of the discipline, has tended to reflect concerns generated within the wider, more autonomous field that has evolved its own institutional structure, issue nexus, and self-image.

This situation has sometimes been perceived as a problem, but it has seldom occasioned great concern among either political theorists or political scientists as

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a whole. In some quarters, both in political theory and mainstream political science, there is a sense of relief that the tension between field and subfield that had characterized the behavioral era has been ameliorated by intellectual distance. There are narrow but weighty professional pressures that tend to lead to the validation of separatism but that are rationalized in terms of notions of intellectual pluralism. Often the problem is depreciated by noting that it is more apparent than real if one takes into account the manner in which the policy turn of the postbehavioral era has produced a convergence between normative and empirical research. But the propensity to confront the issue on a superficial level conceals both the historical source of the problem and some very basic difficulties that cannot easily be dismissed.

A readily accessible and familiar explanation of the alienation of political theory from political science locates the cause in the conflict that arose in the course of the behavioral revolution and its attack on the study of the history of political theory as antiquarian and inimical to the development of empirical theory and a scientific study of politics. This explanation is not incorrect, but it is incomplete. It is necessary to reach a deeper historical sense of the genealogy of political theory and to recover some of the more fundamental issues that occasioned the controversy we associate with behavioralism and its aftermath. By the early 1950s some of these issues had already been submerged in the rhetoric and legitimating philosophies that characterized the debate between "traditional" and "scientific" theory. A pivotal transformation in the discourse of political theory, which occurred during the 1940s, was obscured. An understanding of that transformation is also important for exploring the general problem of the relationship between academic political inquiry and politics—a problem that was at the heart of the controversy about political theory as it originally developed.

The Real History of Political Theory

The basic images of political theory of and by which we are now possessed, images of theory as both a product and activity and as both a subject matter and mode of inquiry, have been generated within, and have little meaning outside, the language of political theory as an academic field. Attempts to endow political theory with world-historical significance by seeking its past in a great tradition from Plato to the present, by categorically defining it as the reflective and critical dimension of political life, by understanding it as a tributary of theory in the natural sciences, or by creating other putative identities that would enhance the authority of this professional field and provide foundations for its claim to knowledge, cannot withstand much analytical scrutiny (Gunnell 1986). Although we have become accustomed to thinking of the history of political theory as the chronologically ordered canon of classic texts, such history is in fact largely a reified analytical construct. What we might call the "real history of political theory" is the history of the academic field that created this image as its subject matter and projected it as its past. Furthermore, despite the current distance between political theory and political science, these images, and the field of political theory itself, were largely an invention of U.S. political science.

Although it is important to investigate the early development of the concept of political theory as well as the academic practice that gave rise to it (Gunnell 1983), the current species of both essentially came into existence after 1940 in the wake

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of the emigration of refugee scholars from Germany. Although the émigrés did not, for the most part, come to the United States understanding themselves as political theorists, this eventually became their identity. The adoption, appropriation, or discovery of this identity and the propagation of their image of, and concerns about, liberalism, as well as a related conceptual cluster including positivism and relativism, precipitated a scholarly and ideological conflict within political science that was manifest in a debate about political theory. Although the behavioral revolution was hardly one-dimensional, it was in an important respect initially a conservative rebellion catalyzed by, and directed against, the encroachment of a vision that was hostile to the traditional values of U.S. political science.

Notions of theory as the history of political ideas and as part of empirical social-scientific inquiry had, through the 1930s, been consistently understood as either complementary or even merely diachronic and synchronic modes of the same endeavor, which involved explaining politics by linking behavior and ideas. Charles Merriam, despite his emphasis on jettisoning or surpassing history as a form of inquiry, neither disparaged the paradigmatic studies of the history of political theories associated with his mentor, William Dunning, nor perceived any serious discontinuity between such studies —in which he himself participated significantly—and the advancement of social scientific theory. But a more significant factor in explaining the lack of tension was an underlying intellectual consensus that transcended various scholarly and ideological divisions between empiricism and idealism, history and science, and statism and laissez-faire. The history of political theory was understood by all as the history of the development of scientific knowledge about politics and as a story of the progress of the symbiotic relationship between such knowledge and the evolution of liberal democratic thought and institutions.

The notions of political theory and the disciplinary practices that developed between the late 1800s and the 1930s were, to be sure, the receptacle for the discourse that emerged in the 1940s. The arguments, for example, of someone such as Leo Strauss about the rise and decline of the great tradition would not have taken hold if the basic idea of the tradition had not already gained conventional acceptance in the work of George Sabine and his predecessors, and there was nothing in the behavioral credo that had not been already articulated as an ideal to which political science should aspire. But the transformation was dramatic. The dominant consensus was frontally challenged for the first time, and political theory, as the focus of controversy, became an essentially contested concept.

The great change began almost unnoticed, or at least uncomprehended, as the claims of Strauss, Hannah Arendt, Hans Morgenthau, Theodor Adorno, Eric Voegelin, Franz Neumann, Arnold Brecht, Max Horkheimer, Herbert Marcuse, and others, in varying ways and degrees but inexorably, reshaped the discourse of political theory. Despite some very great differences in ideological and philosophical perspective, they looked much alike to the successors of Merriam as they uniformly and on very similar bases challenged the liberal, scientific, relativistic, historicist perspective that dominated political theory and political science. What might be taken as the politically conservative arguments of individuals such as Voegelin were initially more professionally visible than those on the Left associated with the Institute for Social Research, but they all propagated the thesis that liberalism, either inherently or because of its degenerate condition, was at the core of a modern crisis and implicated in the rise of totalitarianism.

It would be far too simplistic to suggest

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that the behavioral revolution should be understood exclusively as a reaction to this challenge, but we have lost sight of the degree to which its arguments were originally formed in response to it. The attack on liberalism and science and the rejection of the progressive-pragmatic vision of history were too basic to allow any syncretic resolution. The sense of liberal givenness, which characteristically allowed U.S. theorists to embrace relativism and the idea of the separation of facts and values while remaining totally committed to definite political ideals, was incompatible with the transcendental speculation and philosophies of history, whether of the Marxist or natural-law variety, that marked the new literature. Since at least the turn of the century, such ideas had traditionally been specifically what most U.S. political theorists had understood themselves as rejecting.

I seek to specify as precisely as possible how the controversy commenced and how it shaped the discourse of political theory. The situation as it first emerged was not clear to many of the participants. The new ideas had often not taken distinct and published form when the conflict began, and some of those involved in the controversy were at first truly perplexed. Individuals such as Sabine, whose influential analysis of political theory was not significantly different from that of those devoted to empirical political science, found himself the target of those whose ideas he largely shared simply because he represented the history of political theory. Similarly, protobehavioralists and the founders of the behavioral movement, such as David Easton, saw not only a growing tendency toward historical and evaluative analysis that was pointedly at the expense of the idea of a scientific study of politics and liberal values but one that was being mounted within the genre of political theory, which had heretofore reflected the constitutive conventions of the discipline of political science.

The Transformation of Political Theory

Apart from textbooks, there was at the beginning of the 1940s, a dearth of literature distinctly understood as political theory, and institutionally, as a subfield of political science, it had all but disappeared. The revitalization of discourse could be construed as beginning with the publication of Sabine's 1939 article, "What is Political Theory?" in the first issue of the *Journal of Politics* and with the appearance, in the same year, of the *Review of Politics* (which became a principle vehicle for the work of the émigrés). The resumption of discussion about political theory and its place in political science was in part prompted by an increasing concern about the status of the scientific study of politics which, despite the work of Harold Lasswell and others connected with the Chicago school during the 1930s, had received limited attention after Merriam's enthusiastic claims in the 1920s. More than a decade before the behavioral revolution, Benjamin Lippincott (1940) argued that in practice the discipline of political science was much as it had been at the turn of the century and that the greatest deficiency was theoretical. What passed as political theory had been devoted to the recounting of past ideas, and even among those committed to science, there was an "aversion" and "hostility" to theory (p. 130). But most important was the renewed reflection on liberal political values and the beginning of the impact of newly arrived European scholars who severely questioned the scientific faith and liberalism as well as the historical vision that united them.

A pointed "challenge" to political scientists was advanced by William Foote Whyte in an article published in 1943. This piece (quite accidentally) served as the principal catalyst for the incipient controversy. Whyte was a social anthro-

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pologist who had been studying political organization in a slum district, and there is no indication that he had any notion of the nerve endings that would be agitated by his comments. He suggested that the war had occasioned a concern about democratic values that had led political scientists to "write political philosophy and ethics" and neglect the study of "plain politics." He argued that "a scientific study of politics" required "the discovery of certain uniformities or laws" and that political scientists should direct their attention to "the description and analysis of political behavior" (pp. 692–93).

The most immediate response to Whyte was by John Hallowell (1944a) whose work would become one of the principal conduits through which the ideas of the émigrés entered political theory and whose voice would come to represent the new mood in the field. The response, however, moved the discussion to a very different context. Whyte, much like Lippincott, was writing from the perspective of traditional American social science, but Hallowell identified this critique, as well as the general commitment to science in the discipline, with an intellectual position that was not only alien but unfamiliar to most U.S. political scientists. Hallowell suggested that Whyte's views reflected "increasingly positivistic" trends that threatened to undermine "all belief in transcendental truth and value" that could serve as a barrier to intellectual and political "nihilism" (pp. 642–43).

The political-theory research committee of the American Political Science Association had been largely inactive in the early years of the war, but in 1943 it met, under the chairmanship of Francis G. Wilson, and attempted to sort out the issues that were beginning to surface. Wilson noted that there was now a "deep cleavage among political theorists in the area of primary ideas" on an "ultimate issue" (Wilson et al. 1944, 726–27). The exact nature of this issue and cleavage was something that he had some difficulty in specifying. One facet of the issue, or one way of stating it, involved a conflict between those who, like the "great political thinkers," took metaphysics seriously and those who believed that philosophy was relevant only as "logical thought." Was scientific "detachment" from traditional philosophy a sign of "progress" or "ineptitude"? Another definition of the problem was in terms of a clash between the "theological approach" and the "empirical" or " 'positivistic,' scientific, or liberal technique of social study." A theological perspective was considered by some as necessary for understanding both the past and contemporary society, but there was a question of what constituted such a perspective and the manner in which it might conflict with the "approaches of idealistic and rationalist liberalism" that were more characteristic of political science (p. 727).

There was also a recognition of a conflict between those who stressed "value-free discussions in political science" and those who believed that there was "more in politics than simply clinical observation." All agreed that it was important to "formulate and criticize values" and that there should be "a frontal attack" on the problem of "value in American political society," and many believed that in some sense it was necessary and possible to arrive at "valid social and political principles." There was much the same kind of discussion about knowledge of regularities regarding "political behavior." Most were willing to compromise at some point and acknowledge that both "utopia" and the "facts" should be given consideration (pp. 727–28, 730), but it was clear that the consensus in political theory was breaking down.

Some believed, Wilson reported, that the fundamental cleavage was manifest in the differences between the thought of the Middle Ages and that of the modern age, between natural law and natural rights.

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There was also a division between those who argued that the United States needed a consciousness of history and that an examination of our philosophy of history was in order and those who wanted to concentrate on practical questions of political choice and the "ends-means relationship." There was also a general, but maybe less than enthusiastic, agreement that there was a need for a greater availability of the "texts of the great thinkers" and that "the political tradition of the West must be subjected to close scrutiny." The "ancients" should be studied as really "modern" and "timeless" because we belong to the same tradition but maybe also because there is an "essential nature of moral man or the moral universe (or even Satanic man and the Satanic phase of the universe)" to be discovered (pp. 730–31).

Wilson, who was a specialist in American political thought and a consistent spokesman for a U.S. brand of conservatism, did his best to report and make sense of the various and sometimes novel notions of theory that surfaced in these discussions and to reconcile them. Neither task was easy. The issues being formulated were radically different from those that had been characteristic of American political science and political theory.

In the symposium, Benjamin Wright (who had written on the American tradition of natural law) claimed that the greatest need of the period was for a statement of objectives in terms of ideals, since there was no "clear conception of what we are fighting for, what goals we should seek to attain, even in this country, after the war." We had "been too inclined to hide timidly behind the excuse of objectivity" (pp. 739–40). In this respect, he maintained, interpretations of the history of political thought and analyses of current doctrine could make great contributions to the future of democracy in the United States. Ernest S. Griffith, on the other hand, argued that "research in political theory hitherto has been largely synonymous with searches for the origin, growth, and decline in ideas, principles, and doctrines," while what was required, if there was to be "precision" in the field, was more attention to "the basic concepts that underlie all theory" and to the definition of these concepts. Such research, he cautioned, should not be understood as a search for *"correct"* concepts, since theorists have too long looked for absolute principles and failed to recognize that principles are subjective and historically relative to various institutional arrangements (pp. 740–42).

The claims of Wright and Griffith could easily be situated within the characteristic discourse of the field and did not necessarily represent sharply conflicting attitudes. Wright's notion of natural law, for example, was largely functional or instrumental. The increasing tension between such positions, however, indicated a different kind of influence. Voegelin, who had fled to the United States in 1938, was beginning to make his mark on professional discussions of political theory during this period, and in this symposium he presented at some length the basic research scheme and thesis regarding the derailment of modern political thought that would inform his *New Science of Politics* (1952)—a book that would epitomize one of the poles of political theory during the 1950s and 1960s.

Voegelin suggested that the study of the history of political ideas had been represented with distinction by individuals like Dunning and Sabine and was of particular importance to U.S. scholars who had done the most to develop this field, over which they had held a "monopoly." He claimed, however, that it was now necessary to rethink the field in light of the vast new historical knowledge available and in terms of the general philosophy of history. Dunning, he argued, had made the first real step beyond earlier work by focusing on "political theory" as a special

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category, but his work was somewhat distorted by his emphasis on the notion of "progress." Sabine had taken the further step of making political theory distinctly subordinate to the structure of political history, and it now remained, Voegelin suggested, to pursue this task in a much more "philosophical" manner (pp. 746–51).

The increasingly pivotal issue of relativism was, at this time, grounded in a concrete concern about the defense of liberal democratic principles. The war and the domestic political crises of the 1930s that preceded it had prompted an uneasiness about the lack of an articulate and philosophically grounded democratic ideology. And the work of individuals like Charles Beard and John Dewey had already incited a controversy about historicism and relativism as a threat to both political values and scientific objectivity. The concern about relativism, then, was not without precedent. But as the issue entered the literature of political theory, it took on a reconstituted identity based on new claims about the origins of totalitarianism. The assessment advanced by émigré scholars, and by Hallowell, was that the problem of totalitarianism was at root a problem of science, liberalism, and relativism and that it was susceptible in large part to a philosophical or religious solution.

If we want to understand fully the behavioral animus toward "traditional" political theory, it is necessary to take account of the extent to which what had actually been traditional in American political theory was being fundamentally challenged. The unhappiness with liberalism and science that would mark much of the major work in the history of political theory by the 1960s (Strauss, Voegelin, Arendt, and others) was already taking shape. And it propagated the image of a crisis and decline in Western political history and thought. Hallowell, in reviewing Brecht's account of the decline of the German republic, argued (1944b) that Brecht did not sufficiently stress the degree to which that decline was "a direct consequence of the liberal's lack of conviction in his own philosophy." The solution, for Hallowell, was a renewal of what he believed were the religious and spiritual foundations of liberal democracy.

Hallowell had developed this thesis in an earlier article (1942) and book (1943) in which he argued that the decline of liberalism as an ideology and the rise of totalitarianism were directly related to positivism and historicism and that a reconstruction of liberalism could be effected by a grounding in modern theology as characterized by the work of Niebuhr, Tillich, and Berdyaev. Voegelin reviewed the book sympathetically—suggesting only that Hallowell might seek the roots of the decline of liberalism further back than the nineteenth century. Voegelin was at this time working on a general (but later temporarily abandoned) history of political ideas that attributed the decline of the West, which culminated in Marxism, to the secularization of Christian history during the Enlightenment (1975). Where the tradition of political philosophy went wrong and led to the decadence of modern politics and political thought would become the principal concern of a new wave of historians of political theory.

Hallowell's attack on liberalism became strident (1947), and the substance of his argument was hardly congenial to American political science. Merriam, for example, had stressed the danger of theology to liberalism while Hallowell was suggesting that secular liberalism, with the disintegration of religious conscience, led to anarchy and tyranny. Much of the tradition of U.S. social science had grown out of an attempt to replace religion as a cohesive social force with a science of social control and public policy that would realize liberal values, but now "political theorists" were claiming that

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liberalism and "the liberal science of politics" were a failure and the breeding ground of totalitarianism.

It would be a mistake to assume that political scientists committed to the scientific study of politics were unconcerned with values—either as an object of study or as premises and goals of empirical inquiry. As Herman Finer put it (1945), the need was for an ideology that would accomplish for democracy what Marxism had done for Soviet Communism" (p. 239). These claims would be overshadowed by the renewed commitment to scientific inquiry by the end of the decade, but the "value-free" stance of behavioralism did not entail a rejection or neglect of what it understood as liberal democratic values. Gabriel Almond, for instance, who had studied with Merriam and Lasswell, spoke out strongly at this time for the ethical involvement of political scientists and against Whyte's suggestion that values should be the province of the philosopher. Almond even argued that the "chief challenge" of the day was to seek "a valid theory of natural law" (Almond et al. 1946). The problem was not that the new influences in political theory stressed values but that behavioralism could not countenance the idea that science and liberalism exemplified social and intellectual decline.

The symposium, "Politics and Ethics," in which Almond participated was conceived as an attempt to explore further the issues that had been raised by Whyte. But, for the most part, the participants talked past one another, and the arguments were by now far removed from Whyte's actual concern and understanding. Whyte claimed that he did not recognize the target of Hallowell's criticism—that he had never even met anyone who represented the position attributed to him. He stated that he was not interested in the "philosophy of science" and "positivism" and that Hallowell had gone off on a "tangent" and created a "straw man" (p. 301). Hallowell was caught up in a realm of ideas and issues that were distinctly European and closely related to the arguments of individuals such as Brecht. Brecht's work constitutes a particularly interesting and illuminating case. Although he posed the problem in a paradigmatic form, his solution was not quite consonant with the claims of Voegelin and Hallowell.

Brecht had been a political actor and administrator who did not enter academia, or what he understood as the pursuit of "science," until he came to the United States and the New School of Social Research in 1933 after a period of Nazi harassment. Around the turn of the decade, he published several articles dealing with the question of relativism, absolutism, and science that eventually became the basis of his book, *Political Theory: The Foundations of Twentieth-Century Political Thought* (1959). Brecht did not directly attribute the rise of national socialism to a philosophical context, but, like many others, he was concerned about the influence of legal positivism and relativism on judges and lawyers who applied laws that were morally reprehensible. He believed, however, that it was impossible to seek a form of "scientific" proof or some rationalistic equivalent for moral principles and natural law. Moral judgment was a matter of human volition even though no less lacking in validity. Brecht argued that scientific value relativism, as expounded by Max Weber, Hans Kelsen, Gustav Radbruch, and Lasswell, had once seemed a liberating philosophy, but in the face of Nazism it became a paralyzing one. In a search for intellectual authority to combat totalitarianism, intellectuals, however mistakenly, "turned from being political scientists to becoming ethical philosophers or theologians (if not simply bad logicans) and *they called this science*" (1970, 434, 490). For Brecht, the great dilemma of modern political theory was

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that there was no scientific knowledge of values. His solution was to teach "the *limits* of science" and urge other ways to bridge the *is* and *ought* (p. 494). He was an intensely religious Lutheran, particularly in his later years, and in his own way he attempted to ground political theory in faith and religious values.

In 1947, Brecht reported on a roundtable ("Beyond Relativism in Political Theory") that included Francis Coker, Roland Pennock, Lippincott, Wright, Voegelin, Hallowell, and Almond. Kelsen was unable to attend, but he sent a message indicating that despite the growing unpopularity of value relativism in the United States, he was still an adherent and remained a critic of moral "absolutism." Kelsen's legal positivism was a prime target of those such as Hallowell who wished to ground political theory in religion and natural law, but for Kelsen, as well as for most U.S. political scientists and theorists, value skepticism and relativism were understood as the foundations of liberal democratic theory. Only on this basis, they believed, could liberal democracy be justified. Moral absolutism signaled political absolutism for émigrés like Kelsen, while for Kelsen's erstwhile student, Voegelin, relativism abetted and reflected the decline of the West.

Brecht suggested that the paradox of modernity was that "modern science and modern scientific methods, with all their splendor of achievement, have led to an ethical vacuum, a religious vacuum, and a philosophical vacuum." He argued that the situation had come to a point where social science found it impossible to distinguish between right and wrong, good and evil, and justice and injustice. Political science was particularly affected by this dilemma, because it dealt most directly with the phenomena of nazism, fascism, and communism, which had "settled down" in this vacuum. Brecht claimed that "no political theorist can honestly avoid the issue" posed by "scientific relativism" (pp. 470–71). Most participants in the symposium offered little in the way of a concrete recommendation for getting "beyond relativism." Hallowell made his characteristic case for Christianity as the basis of political theory, but Wright and Lippincott, speaking for the more traditional conception in American political science, warned against any such retreat to commitment.

There has not been sufficient recognition of the degree to which the transcendental urge and historical pessimism manifest in the émigré literature affected political theory. It contributed to the split between political theory and political science during the behavioral era and to the eventual constitution of political theory as an independent field. Although the behavioral revolution, or counterrevolution, in many ways transformed the mainstream practice of the discipline, it was, at least theoretically, a reaffirmation of past ideals—ideals that did not rest easily alongside the arguments that characterized the new literature of political theory. Although the behavioralists singled out the study of the history of political theory as requiring displacement, transformation, or supercession, this was more because it was the genre to which most of the offending arguments happened to belong than because the literature characteristic of the field before 1940 actually embodied the problems with which behavioralists were concerned.

By 1950, there was an antiscientific, antiliberal sentiment in political theory even though neither the medium nor the message were always entirely clear. Although, for example, Arendt had published widely during the 1940s in a variety of journals, her first major work in English, *The Origins of Totalitarianism*, did not appear until 1951. Much of Strauss's *Natural Right and History* was originally presented as Walgren lectures at the University of Chicago in 1949, a year after his appointment, but the book

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was not published until 1953. Strauss had already announced in his work on Hobbes (1936) some of his distinctive themes, including the depreciation of both liberalism and the new science of politics, but the book received little attention and surely not much comprehension. The same could be said of Herbert Marcuse's *Reason and Revolution* (1941) and Horkheimer's *Eclipse of Reason* (1947). In an article-length review of Strauss's book, Carl Friedrich, himself an earlier émigré, praised the effort but clearly did not understand the project. He wished that Strauss had been more forthcoming about his own views, but he believed that it was safe to conclude that Strauss was a "historical relativist" (1938).

Although these attacks on science, liberalism, and the legitimacy of the modern age had not yet coalesced into a definite oppositional force in political science, they were at least engendering an alien mood that had a considerable professional impact. There were all sorts of reasons to reassert the scientific image of political science in the postwar years, and the new voices in political theory were not only failing to do so but were inimical to such an image. The need for scientific theory was the theme of William Anderson's survey of political science in 1949. What was required, he argued, was an "established body of tested propositions concerning the political nature and activities of man that are applicable throughout the world and presumably at all times" (p. 309). It was, he claimed, necessary to distinguish between politics and science and pursue the latter, and the way to do this was to study individual "human political behavior" or the "political atom" and produce knowledge in the "field of scientific method" (pp. 312–14).

Behavioralists, beginning in the 1950s, would increasingly emphasize their dedication to a vision of pure science, but the underlying concern at the end of the decade was still the articulation and realization of a science of *liberal* politics. In two articles, on Bagehot (1949) and Lasswell (1950), that closely preceded his analysis of the "decline of political theory" (1951), Easton made clear that the pursuit of science was a means and not an end and that the belief in the complementarity of scientific realism and liberal democracy was still alive. The ultimate purpose remained political rationality, but the immediate goal was to create a theoretically grounded science of politics equal to this task. The priority for the moment was an emphasis on what Lasswell and Abraham Kaplan termed the "contemplative" as opposed to the "manipulative" dimension of science (1950).

The individuals most responsible for affecting and sustaining the behavioral revolution had almost without exception been primarily trained in the field of historical and normative political theory, as traditionally conceived. While the debate within the discipline would eventually often be understood as a conflict between political *scientists* and political *theorists*, it really signified a split between different conceptions of theory. Few of the early behavioralists understood themselves as antitheoretical, and probably very few initially understood their concern with scientific political theory as a rejection of their earlier education. To some extent, what was involved was a lag between the graduate educational establishment and the new literature and mood of political theory. The theorists trained in the 1930s and 1940s had, for the most part, not been exposed to the arguments of the new wave of European thought that was appearing in political theory, and they felt a good deal more at home with the traditional goals of the field than with the perspective that characterized this literature. Ultimately they felt compelled to choose between political theory and political science, and the choice was as much ideological as professional. But by the mid-

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1950s, the behavioral critique of political theory was forcing the same polarizing choice on theorists concerned with historical and normative issues.

In 1950, Lippincott attempted, as he had a decade earlier, to make sense of what constituted political theory in the United States and to provide a critical analysis of the field. His discussion did not gain the attention received by some of the testaments of the behavioral movement that would be advanced during the next few years. But it indicates the degree to which the basic tenets of behavioralism had already been formulated and enunciated by individuals like Lippincott, who were hardly enemies of "traditional" political theory but who were clearly worried about the direction that the enterprise was taking and about its relationship to political science.

Lippincott suggested that political theory was at least potentially—and as correctly understood—the "most scientific branch" of political science. It was, however, an area in which political scientists had "produced little," if theory were defined, as he believed it should be, "as the systematic analysis of political relations." It should, he argued, be concerned with "general" claims about the "actions" and behavior of men" and with particular events and institutions only as objects for testing its principles (pp. 208–9). Up to this point, Lippincott argued, "the greatest effort has been devoted to writing the history of political ideas" and "defining and classifying terms and principles of politics." The methods in political theory had been basically historical, and the "emphasis placed on the history of political ideas has meant very largely the abandonment of the aim of science" (pp. 209, 211, 214).

The call for science was increasingly accompanied by an attack on the existing practice of political theory. Theory was often described as historical, teleological, utopian, moralistic, ethical, and generally obscurantist. There was more going on in these critiques than was readily visible, and the exact target of the criticism was often not clearly or easily specified. By 1950, however, Hallowell had brought together his arguments in a comprehensive work (*The Main Currents of Modern Political Thought*), and to understand fully the image of political theory that behavioralism would invoke, this work must be examined. It reflected and summarized claims that had emerged in the discourse of political theory during the 1940s, and it initiated a genre that would find its classic expression in the work of Strauss, Voegelin, and others during the next decade. Even a work such as Sheldon Wolin's *Politics and Vision* (1960), which ideologically and philosophically was far removed from the positions of Hallowell, Voegelin, and Strauss, was still a story of liberalism and the decline of modern politics. The imprint of the 1940s was fundamental for the future of political theory.

Although Hallowell's book was about "modern political thought," a large portion was devoted to a synoptic reconstruction of the development of political ideas from ancient times up through the emergence of what he designated as "integral liberalism," which he claimed was an outgrowth of the ideas of individuals like Grotius and Locke. The image projected was one of a seamless web of history that moved from the classical world through the evolution of modern science, the Renaissance, and Hobbes to the rise of liberalism. However, the story of modern political thought was now a story of decline—a decline as organic as the upward slope of the past and one that was offered as an explanation of modern political problems and "the crisis of our times" exemplified in socialism, Marxism, the Soviet Union, fascism, and the theory and practice of totalitarianism in general. But the most basic problem was the decadence of contemporary liberalism and its inability to defend and maintain itself.

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In Hallowell's saga, the main villain was positivism, which in turn he viewed as a direct outgrowth of the Enlightenment, utilitarianism, and German idealism. His notion of positivism was the very general one of an "attempt to transfer to the study of social and human phenomena the methods and concepts of the natural sciences," and he believed that it was represented by the work of Comte, J. S. Mill, Herbert Spencer, Frank Lester Ward, and Gumplowicz. Positivism, with its attendant separation of fact and value, and relativism undermined liberalism and allowed the rise of antiliberal political movements. For Hallowell, the answer to the modern crisis was once again Christianity and the reconstitution of decadent liberalism on the basis of religious values. This particular work, although reasonably influential in political theory, was seldom the specific focus of behavioral criticism, but it represented the kind of claims that in large part evoked that criticism.

Some émigrés like Kelsen (1948) and Felix Oppenheim (1950) would continue to argue, like U.S. liberals, that relativism and empiricism were more conducive to democracy than moral absolutism. They claimed that there was a root connection between democracy and the spirit embodied in the scientific method. The imminent debate between "scientific" and "traditional" political theory would be far from simply a debate about method and political inquiry or even about an emphasis on values as opposed to facts. It was, in the end, a debate about liberalism, its foundation, and its fate—and about the relationship between political theory and politics.

By the 1950s, the dispute in political theory was projected on the wider screen of academic philosophy and issues about the nature of science. This was already quite evident in the 1950 volume of the *American Political Science Review*. J. J. Spengler, speaking from the perspective of an economist, claimed that it was time that political science was "transformed into a full-bodied social science" (p. 375). What was required, T. I. Cook argued in a discussion of this thesis, was the development of a general unified science of theories and postulates similar to that "developed in natural science before, and first completed by, Newton" (p. 388). Wilson, commenting on these claims, observed that U.S. social science had now come to share a common conception of theory, knowledge, and social reality that was based on scientism, empiricism, pragmatism, instrumentalism, positivism, relativism, and operationalism.

In a symposium on "The Semantics of the Social Sciences," which was basically a statement of the philosophical and social-scientific position of the Chicago school, Charner Perry, a Chicago philosopher and long-time editor of *Ethics*, argued that natural science had made its great advances by going beyond the lore and common sense embedded in "anthropomorphic and teleological" language. In political science, however, there still was "no important contribution in the field resulting from the application of scientific method." This could be attributed to the "theological, normative, or even moralistic terms" of "political theory" which "belong to subjective or fictitious universes of discourse quite inappropriate to a general science of society" (1950, 397–99).

There was an increasingly extravagant dimension to the rhetoric on both sides of the debate. Actual research and publication in the field hardly matched the picture of "positivism" that was painted by the critics of scientism, and the image of moralistic political theory on which the protobehavioralists focused was equally unreflective of the literature. To claim, as Perry did, that political science was "mostly history and ethics" was simply counterfactual, and to argue that "the propositions of political theory have a

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character of 'unreality' and futility that bars out any serious interest in their discussion" (pp. 399, 401) was clearly to speak generically on the basis of a narrow and unspecified body of literature. But there was, ironically, a sense in which each faction was now beginning to fulfill the image conjured up by its opponents.

Herbert Simon, George A. Lundberg, and Lasswell participated in the discussion and pressed, as they would in many forums, for making the language of social science scientific. Although they did not specify their sources, they were clearly no longer strangers to the positivist philosophy of science. Simon enunciated a model of science as a system of predictive-cum-explanatory general propositions from which could be deduced other propositions about concrete observables that were therefore testable. Science was conceived methodologically as a unity, and human action as an object of inquiry was viewed as requiring no departure from the methods of natural science.

For Simon, it was essential that political science adopt what he considered to be the more advanced methods of the other social sciences, and he insisted that central to this task was a *transformation and redefinition of political theory* that would entail "consistent distinctions between political theory (i.e., scientific statements about the phenomena of politics) and the history of political thought (i.e., statements about what people have said about political theory and political ethics)" (p. 411). Lundberg, a sociologist, who was a major advocate of positivism in the social sciences during this period, confirmed Simon's image of science and stressed the need to "adopt the orientation of modern natural science," which would involve as a first step finding out "what the methods of natural science are" and thus provide a means for getting away from the a priori philosophy characteristic of the humanities (p. 421).

Lasswell warned against getting too involved in the question of what "science" means and neglecting the practical end toward which scientific inquiry was ultimately directed, that is, "to decrease the indeterminacy of important political judgments," but he advocated "more attention to the construction of theoretical models" that would guide research (pp. 423, 425). Exactly what was entailed by the commitment, or recommitment, to science was not very clearly defined, but there definitely was a new philosophic dimension to the argument. Lasswell's collaboration with Kaplan, who had been a student of Carnap's, was one obvious point of intersection. And they had stated in the introduction to their book (1950) that it reflected a new scientific outlook informed by "a thorough-going empiricist philosophy of the sciences" based on "logical positivism, operationalism, instrumentalism" (pp. xiii–xiv). Maybe their position was actually more justified than informed by this philosophy, but in the face of new philosophical challenges, political scientists attempted more self-consciously to ground and articulate their scientific faith.

By 1950, some mainstream political scientists were becoming uneasy about the implications of the new scientism. Paul Appleby (1950), speculating on the direction of political science during the next 25 years, argued that a much broader value-oriented vision was required than the one that was then developing in the discipline, and for many the concern was still to link science to problems of practical politics (White 1950). Easton's call for the reconstruction of political theory has quite reasonably often been construed as the first shot fired in the behavioral revolution. But although *The Political System* (1953) was, to be sure, a critique of the study of the history of political theory and a plea for the creation of scientific theory, it is sometimes forgotten that it was an explicit challenge to the new "mood" in political theory and an attempt

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to find a basis for the reconciliation of political science and political theory that would at the same time sustain and advance liberal democratic values (chap. 1). By mid-decade such mediative integrating efforts had largely given way to the alienating forces that pushed political theory and political science along different paths. And in an important sense the language and agenda of political theory, for at least a generation, had already been set.

Conclusion

The literature of political theory is, and since the late 1930s has been, saturated with discussions about liberalism and its tradition, rise and decline, faith, dangers, limits, collapse, challenges, agony, paradox, irony, spirit, development, end, poverty, and crisis and its relation to innumerable things, individuals, and other political concepts. Even when the concept is not specifically a focus of discussion, the concern about liberalism significantly structures the discoursive universe of political theory. An understanding of this situation requires a grasp of those initiating issues that informed the invention of political theory and that fundamentally shaped its real history. The enthusiastic reception of European thought in recent years, such as that associated with Habermas, is directly related to its perpetuation of the critique of liberalism, and surely some of the literature that has had the greatest impact on the field has been devoted to attempts, such as that of John Rawls, to provide new foundations for liberal theory.

Although the tension between political science and political theory was, and in large part has remained, the product of a disagreement about liberalism, the controversy quite early on became detached from concrete political issues. From the beginning, there was a tendency for the debates about liberalism to escape the political concerns that had given rise to them. For the émigrés, the experience of Weimar and totalitarianism was determinative and came close to confirming the idea that in Germany, as in Athens and Rome, liberalism was the manifestation of political decline and the threshold of tyranny. No matter how much individuals like Strauss and Marcuse might disagree on various grounds, they were at one in extending the analogy to contemporary politics in the United States. And even their specific critiques of liberalism were not all that different. For U.S. political scientists also, the rise of totalitarianism and its attendant ideologies during the 1930s, as well as the domestic problem of rationalizing the conflict between government and the economic enterprise, indicated a need to rearticulate and ground the principles of liberal democracy. If a sense of crisis regarding liberalism, in theory and practice, had not been part of the world of political science, the arguments of the émigrés would likely not have carried the weight that they did.

On both sides of the controversy, however, there continued to be an alienating displacement of the issues. In the case of the émigrés, there was a philosophization of their experience and a projection of the analysis onto recalcitrant political circumstances in the United States as well as the ambiguous screen of world history. This produced an estrangement between academic political theory and politics that has plagued the subsequent evolution of the field. The defense of liberalism among political scientists also became oblique. Either it became the validation of an image that was not easily related to political realities, or it took the form of a philosophy of science, conceptual frameworks, and empirical findings that often seemed to confirm the existence and efficacy of liberal society.

The debate about relativism was also originally grounded in a practical issue

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that was eventually obscured by the philosophical formulation of the problem. This was the problem of the relationship between academic and public discourse or between philosophy, political theory, and political science on the one hand and politics on the other. This concern about the authority of political inquiry and the intellectuals who engaged in it was equally prominent in the work of both the émigrés and the practitioners of traditional American political science, but the responses were different.

Although relativism, as a philosophical problem, may arise in reaction to crises in substantive practices such as science and politics, it more basically and frequently reflects a crisis in the understanding of the relationship between first- and second-order discourses, that is, between, for example, academic political theory and politics. It is a problem in part because second-order inquiry conceives its mission as the proprietory one of discovering, validating, or explicating the grounds of judgment in the activity it has appropriated as its subject matter. But the problem is often only tangentially one of the possibility and integrity of practial judgment. The pursuit of a solution to the problem of relativism is a quest for a comprehensive answer to the infinitely complex and contextually diverse questions attending the nexus between theory and practice. It most essentially reflects a crisis in the self-image of political theory and the foundations of its claims to knowledge.

The world of the émigrés was one of not only political but intellectual and professional insecurity, and the problem of relativism was a philosophical expression of their anxiety. The question of the authority of academic inquiry was an important concern for political science, but there were several reasons why the discipline tended not to perceive relativism as a threat. Easton's remedy for the decline of political theory, for example, called for the reconstitution of value theory without jettisoning the liberal belief in the relativity of values and their emotive nontranscendental basis. One reason that political science was able to absorb the issue of relativism was because it was an essential part of the pragmatic liberal creed of fallibility and progress in both scientific inquiry and politics. But the liberal science of politics had its own grounds of certainty.

There was, first of all, the belief that history validated liberalism and that liberal values were embedded in the basic institutions of U.S. politics. Relativism never was taken as implying that one value was as good as another but only that values reflected contexts and perspectives. Furthermore, certainty was to be achieved through science. Although science could not yield values as such, it seemed to select, confirm, and discredit them in various ways, and, again, there was the belief that the logic and procedures of science and liberal democracy were inherently mutually supportive and that both rested on a pragmatic notion of truth and the eschewal of absolutist speculation. Finally, political science was, much more than the emerging autonomous enterprise of political theory, an established disciplinary practice with its substantive criteria of judgment and, despite continuing concern about its public role and possibilities, a sense of practical efficacy and professional identity. It was, consequently, less daunted by the philosophical phantom of relativism that haunted émigré political theorists.

The discipline of political science constituted a context that profoundly and singularly shaped the institutional form and ideational content of political theory. Political theory, in turn, was deeply implicated in some of the most fundamental changes that took place in political science, including both the behavioral and postbehavioral revolutions. Today, political theory has largely retreated from an

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involvement with, let alone critique of, political science, and political science has, for the most part, afforded political theory the ultimate disdain of pure tolerance. This situation has allowed many important issues, both explicit and implicit, to slide into obscurity or to become, as in the case of liberalism and relativism, detached from the practical concerns that originally generated them. Professional pressures, under the guise of intellectual principle, continue to push in the direction of separatism, but the results are debilitating. Both political science and political theory have been diminished. The former has lost its most important critical and reflective dimension, and the latter has lost its congenital and maybe most authentic field of action.

Note

This essay is based on a manuscript (in progress) devoted to a study of the history of political theory as an academic field and conceived as a case study of the relationship between academic and public discourse in the United States. Earlier versions were delivered at the Yankee Conference for the Study of Political Thought (Amherst, MA, 1984), the annual meeting of the American Political Science Association (New Orleans, LA, 1985), and the Joint Finnish-International Political Science Association Symposium on the Development and Institutionalization of Political Science (Helsinki 1985). A longer version will be published in a volume edited by Dag Anckar and Erkki Berndtson.

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John G. Gunnell is Professor of Political Science, State University of New York, Albany, NY 12222.

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POLITICAL JURISPRUDENCE, THE "NEW INSTITUTIONALISM," AND THE FUTURE OF PUBLIC LAW

ROGERS M. SMITH
Yale University

For the last quarter century public-law studies have been dominated by "political jurisprudence," which tries to understand law as a product of political forces. Critics claim this outlook, as now articulated, has generated fragmented empirical work disconnected from larger normative issues. This essay uses recent "institutionalist" or "structuralist" perspectives, based on critiques of pluralist political science and both Marxist and functionalist sociology, to propose a political approach to public law studies that can avoid such criticisms. If both empirical and normative public-law scholars took as their central concern the "dialectic of meaningful actions and structural determinants" and recast their research in several specified ways, they might be better able to describe the role of normative ideas in law and to achieve a broader empirical agenda that could ground and inform normative debates.

In a 1984 symposium, several leading political scientists specializing in public law considered what directions their subfield might take in the future. They generally agreed that a "vision of political jurisprudence," calling for analysis of law and courts as aspects of broader political processes, dominated work in the field. But they perceived a plurality of approaches under this label, some in sharp tension with others. Studies focusing on the interplay of courts and interest groups; the politics of appointive processes; courts as small-group decision makers; the implementation and consequences of judicial rulings; the socioeconomic makeup of bench and bar; statistical-attitudinal analyses of judicial voting behavior; litigative versus nonlitigative means of conflict resolution; comparative studies of legal systems; and much more were all identified as currents flowing within the broad river of political jurisprudence (Danelski 1984; O'Brien 1984; Sarat 1984; Shapiro 1984; Stumpf 1984).

Reflecting on this diversity, David O'Brien (1984, 561) argued that despite its hegemony, political jurisprudence had proven unable truly to unify the subfield into an influential intellectual discipline. He traced this inability to the approach's failure to link "normative jurisprudence and positive political analysis" adequately. Empirical work had splintered and sometimes faltered due to the absence of any theoretical structure that could draw on normative concerns to define an appropriate agenda for political jurisprudence's "descriptive enterprise."

In contrast, Martin Shapiro (1984, 543–44) opposed seeking such a synthesis. He insisted that political jurisprudence had succeeded in becoming the modern orthodoxy even in law schools. Much progress would be lost, he feared, if careful empirical work should give way to a more old-fashioned "jurisprudence of

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values" that would be "really a branch of normative political philosophy." Yet he acknowledged regretfully that many younger scholars were moving in precisely this direction.

The symposium's portrait of the subfield suggests a multiplicity of approaches that threatens to break out into renewed clashes between "behavioralist" and "normative" public-law scholars, now perhaps compounded by intergenerational divisions. This essay argues that any revival of these longstanding feuds is pointless and avoidable. Recent directions in research in a number of fields, of a sort dubbed the "new institutionalism" by James March and Johan Olsen (1984), suggest how public-law scholarship might be recast to unify many of its longstanding descriptive and normative concerns. By explicitly designing their studies as explorations of what Theda Skocpol (1984, 4) has termed "the dialectic of meaningful actions and structural determinants," political scientists with many different interests might pursue them so as to facilitate the communication and comparison of their results not only among each other but with the work of empirical political scientists and normative theorists more generally. That possibility, to be sure, is speculative, but it is worth considering the potential of these recent trends to provide more common ground for public-law scholars.

Two Roads to the New Institutionalism

It is only fair to acknowledge at the outset that much of my own previous work falls squarely within what Shapiro labels, rightly, the *jurisprudence of values.* Even so, I have come to share his concern that public-law scholarship will not flourish if all scholars focus simply on spinning out their own normative legal theories. While the subfield cannot be vital without vigorous normative debates, the ratio of new insights to reworkings of established views will be disturbingly low if disputes over values are all that is on our agenda.

But many recent efforts to reinvigorate qualitative inquiries into norms, values, and ideologies within public-law scholarship do not really represent efforts by legal scholars to be political or moral philosophers. Rather, they are attempts to integrate the study of ideas in law with *descriptive* studies of the historical evolution of political institutions and behavior. These authors regard qualitative studies of the patterns of reasoning characteristic of various strains of legal discourse as investigations into one dimension of actual political conduct—a dimension that needs to be assessed like any other if we are to build up a comprehensive empirical portrait of political life (Gordon 1984, 57–125; Smith 1985, 5–9).

It is true that this focus on values and ideologies derives from several objections to how political and legal evolution have previously been depicted, including reductionist treatments of normative issues. And while many recent writings aim at better empirical accounts of legal ideas, it is also true that they can be quite valuable for normative debates. Much of this sort of descriptive work turns out, incidentally but felicitously, to dovetail nicely with emerging modes of moral argument. Even so, the claim that qualitative studies of the historical role of legal ideas can contribute to empirical political science as well as to moral philosophy deserves to be taken seriously.

A major reason for granting credibility to this claim is that there are two distinguishable roads leading to the study of ideas in law as part of the new, developmentally focused "institutional" analyses March and Olsen describe. These roads take as their points of departure two distinct and important academic strains, one in contemporary political science and one in sociology. Their convergence suggests

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that "new institutionalist" accounts are indeed responding to real weaknesses in current perspectives.

The road March and Olsen stress is a path of reaction against the treatment of legal and political institutions simply as epiphenomena of self-interested individual and group behavior—a treatment dominant in mainstream U.S. political science since the 1950s and visible in much of what Shapiro terms *political jurisprudence.* The other road is one trafficked mostly by neo-Marxist comparative scholars and somewhat like-minded members of the Conference on Critical Legal Studies. These writers are reacting against traditional Marxist and sociological reductions of such institutions to epiphenomena of economic relations or the functional needs of social systems. Instead, they stress the "relative autonomy" of political and legal organizations, including recurring organizations of legal ideas, from all such socioeconomic "deep structures."

As suggested above, the convergence point of both reactions is on the importance of the interrelationship between human "institutions" or "structures" and the decisions and actions of political actors. In these approaches to the study of politics, institutions are expected to shape the interests, resources, and ultimately the conduct of political actors, such as judges, governmental officials generally, party or interest-group leaders, and other identifiable persons. The actions of such persons are in turn expected to reshape those institutions more or less extensively. Ideally, then, a full account of an important political event would consider both the ways the context of "background" institutions influenced the political actions in question, and the ways in which those actions altered relevant contextual structures or institutions.

The term *institutions* as used here has a quite inclusive meaning but no more inclusive than Samuel P. Huntington's definition of institutions as "stable, valued, recurring patterns of behavior" (Huntington 1968, 12). Indeed, a focus on the "interplay of meaningful actions and structural contexts" means that political scientists might plausibly narrow Huntington's definition somewhat (Skocpol 1984, 1). They would normally attend only to relatively enduring patterns of behavior that (1) have arguable importance for human decisions that significantly shape social development and (2) appear subject to meaningful modification through such choices and conflicts. Nonetheless, this definition is broad enough to include not only fairly concrete organizations, such as governmental agencies, but also cognitive structures, such as the patterns of rhetorical legitimation characteristic of certain traditions of political discourse or the sorts of associated values found in popular "belief systems."

Thus these definitions can be useful in analyzing the elements and tendencies of legal and political outlooks or ideologies as well as behavioral regularities more narrowly defined. They also preserve at least the possibility that concrete political choices will prove to have important consequences, intended and unintended, that result not from any shaping "structural context" but from the actors' own creativity. It is their attention to the role of ideas and their assumption of the potential meaningfulness of political decisions that make these approaches well suited to speak to normative as well as empirical concerns. They can provide descriptive materials for the sorts of pragmatist moral arguments increasingly advocated by a diverse range of political theorists. Richard Rorty (1979, 1986), Alasdaire MacIntyre (1981), and Michael Walzer (1983), for example, all agree that philosophers can best assist prudential deliberations on current issues by identifying and assessing our constitutive moral traditions and their historic role in political life.

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Such accounts are precisely what descriptions of legal ideologies as political "institutions" or "structures" provide. Attracted by the promise of such benefits, then, let us begin appraising the arguments for these approaches by examining the two roads to the new institutionalism in more detail.

The Road from Mainstream Political Science

As March and Olsen argue, since the behavioral revolution of the 1950s, much U.S. political science has analyzed politics chiefly in terms of the conduct of individuals or organized groups. Their behavior is normally portrayed as the result of rational calculations designed to advance the individual's or group's self-interest. The "behavior" of political institutions, in turn, is treated as simply the product of complexly interacting individual and group choices and actions. Such analyses tend, moreover, to regard the interests and resources of political actors, and hence much of the content and consequences of political action, as ultimately reflective of deeper social and economic forces which shape politics much more than politics shapes them. Thus the preferences and powers of political actors are often treated as exogenous "givens" in political analysis. (Although many desires may of course be described as internal to persons, those impulses are regarded as determining forces that are relatively immune to political action—so their roots are not analyzed when politics is studied.) And while such preferences or interests might be defined quite diversely, much modern political science presumes that concerns to enhance one's economic condition and political power are usually decisive (March and Olsen 1984, 735–38).

Numerous writers also assume that the political choices born of rationally self-interested calculations usually result in outcomes that are "efficient" or "functional," at least for the dominant elements in the prevailing social and political structures (March and Olsen 1984, 737). As March and Olsen fail to note, however, many critics have assaulted the political science of the 1950s for assuming too complacently that all possible "groups" latent in a society can develop organizations to protect their interests effectively. These writers hold that many significant interests are frequently left inarticulate, excluded, or systematically slighted (Connolly 1969, 13–19; Lowi 1979, 57–63; McConnell 1966, 7–8). Thus they believe the apparent systemic "efficiency" of the decisions of ruling groups is often accompanied by frustration for others, who may ultimately produce significant social ruptures. But after making these important criticisms, only a few of these analysts have gone on to challenge the prevailing picture of politics as largely a matter of self-interested individual and group behavior.

According to the view still dominant in political science, then, politics is comprehended as kind of drama at sea. It occurs on the surface, where individuals and groups sail along, rationally navigating in pursuit of booty, frequently clashing with each other in the process. Their fates, however, frequently depend on the dynamics of the largely uncontrollable economic, technological, and social forces that surround them. The student of politics is mostly concerned with making sense of the calculations and actions of the pilots and crews, but the analysis often rests on showing how they could not safely act otherwise.

In some important respects, this image of political life has only been polished by the development of rational-choice theory into a more formal paradigm for virtually all political and social, as well as economic, analyses, although certain significant departures are visible as well. Leading works in this mode have stressed the constraints that information costs impose

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on decision making, giving this portrait of human conduct more empirical plausibility. They have then explored the interconnections between calculations of individual interest, particularly those of political "entrepreneurs," and the creation of organized self-interested groups. Much attention has also been given to how institutions try (and often fail) to aggregate individual choices coherently under various decision rules, and how these complications affect political calculations in groups small and large. And rational-choice theorists continue to encompass more and more political behavior within such analytical frameworks (Buchanan and Tullock 1962; March and Simon 1958; Mueller 1979; Olson 1965; Riker 1962; Simon 1957).

Many of these analysts do break significantly with more traditional studies of individual and group behavior in at least two respects. They are often unconcerned about whether rational-choice models really *describe* the actual decision processes of human beings in any very precise way. For example, the economist-lawyer Richard Posner (1977, 12–14) follows Milton Friedman in maintaining that all truly scientific theories are abstractions that are "necessarily 'unrealistic' when compared directly to actual conditions." The question should be not whether a theory captures "the full complexity, richness, and confusion" of human behavior, but whether it proves to have "utility in predicting" conduct, so that it can be relied on in designing policies and political strategies. In Posner's view, rational-choice models used by economists and other public-choice theorists have been shown to have "surprising" predictive power, far more than the available alternatives; so they offer the most promising direction for social-scientific research.

Partly as a result of this diminished concern to describe social reality in all its complexity, many rational-choice analysts have also not been greatly concerned to explore how far the array of effective, organized groups in political life mirrors the "actual" structure of group interests in society. Because their models suggest that nearly all groups are products of self-interested political organizers and subsidizers offering incentives that generate group membership and activity, it is the presence, skill, and opportunities of such leaders and patrons that determine whether a potent interest group will emerge. Thus while the disadvantaged clearly face special burdens in gaining organized advocacy, there seems little point in trying to determine what groups, organized and unorganized, might reflect the "real" structure of interests in society. We can best answer questions about why certain interests are and are not represented by keeping our focus on the calculations and resources of these leading political actors (Hansen 1985, 93–95; Salisbury 1969, 11, 23–24; Walker 1983, 402–4).

These differences, however, only take to an extreme certain tendencies in earlier mainstream political science, particularly the inclination to use self-interested calculations of wealth and power maximization as the basic model of political behavior. Thus rational-choice models are not likely to satisfy those who believe that this perspective recognizes at best a narrow subset of possible human standards, and they are even more frustrating to those who believe much political science is methodologically insensitive to the claims of the oppressed. Yet for many, the rapid growth of rational-choice analyses in several disciplines suggests that this general approach to politics will eventually be able to provide more theoretically rigorous and empirically falsifiable accounts of virtually all of political and social life.

Hence we should note that William Riker (1980, 444–45) attempted a particularly relevant expansion of the rational-choice paradigm when he called for a

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"return to the study of institutions," out of recognition that they systematically exclude or include certain "tastes or values." He cited pioneering studies by Kenneth Shepsle (1979) on how a congressional committee system may achieve "structural equilibrium"—a condition in which it is possible to pass motions that genuinely cannot be defeated by any other alternatives—if committee jurisdictions compel the disaggregation of multidimensional issues and if certain other requirements for members' preferences are met. More generally, Riker argued that we must use the tools of rational-choice theory to explore how institutional arrangements, themselves the products of past political choices, act as "congealed tastes" that influence the kinds of values that are "feasible and likely" outcomes of current decision processes.

While such extensions of the rational-choice paradigm to include institutional analyses are important, they do not alter the approach decisively. These writers still contend that one should build up a picture of institutional structures by stressing the individual and group decisions and actions that led to their creation; and they continue to treat tastes and preferences "as given." These tastes are impulses that may be blocked or advanced by different institutional arrangements, but they are otherwise exogenous to institutions, products of other forces that in the long run "cannot be denied" (Riker 1980, 444; Riker 1982, 190). These writers also still conceive of "tastes or values" in utilitarian fashion, as preferences—usually for wealth or power—to be maximized. Hence their focus remains on individual and group calculations of how to achieve values that are thought of as externally determined and described in rather reductionist terms.

It would be more tedious than difficult to show that much scholarship in "political jurisprudence" falls within the broader behavioral approach that since the 1950s has led, among other things, to the proliferation of rational-choice analyses. A few leading examples should suffice. None could be more appropriate than the seminal exposition of that jurisprudence by Martin Shapiro (1964, 7–8), where he called for examination of institutions in terms of "the behavior of their personnel, and their places in the various decision processes." And he identified David Truman's classic effort "to analyze all government in terms of the influence and interactions of interest groups" as a chief "catalyst for the new jurisprudence." Shapiro's subsequent work consistently analyzes judicial decisions as responses to constituent claims and as expressions of power-seeking political actors, as does, for example, Stuart Scheingold's influential essay on the "politics of rights" and the growing literature linking patterns of judicial decisions with patterns of party realignment (Adamany 1980; Funston 1975; Lasser 1985; Scheingold 1974; Shapiro 1978, 1981, 1986). The many empirical analyses that connect judges' decisions to attributes such as their socioeconomic, educational, or professional backgrounds or to "values" treated as external "givens," rest on a similar picture of political actors as calculators advancing preferences or interests that are more the results of powerful sociological forces than their own deliberations (Gibson 1978; Goldman 1982; Rohde and Spaeth 1976; Schubert 1975; Tate 1981).

Recently, drawing on the newer rational-choice mode of analysis, Jeffrey Segal (1984) has employed the contemporary recognition of bounded rationality to describe how judges simplify their choice problems by relying on cues, such as certain facts about each case. At varying levels of formality, other political scientists have used the game-theoretic-style calculations involved in negotiation among multiple members of a decision-making group in the problems of aggregating choices, to clarify judicial strate-

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gies and to determine likelihood of winning coalitions in decision-making processes (Murphy 1964; Rohde 1972).

All this work is valuable, the image of politics that it elaborates is in many ways compelling, and it is at best premature to dismiss the hope that these endeavors will lead us closer to more truly scientific political studies. Nonetheless, the limitations of these approaches suggest that their underlying conceptions of politics and their paradigms for political analysis must be significantly if not fundamentally modified to be fully adequate to human experience. While March and Olsen and others canvass many objections, the root problem is that all these fairly diverse writers treat the resources, the institutional environment, and especially the very values and interests of political actors as exogenous, as determinants of political-choice situations that shape events while remaining more or less impervious to conscious human direction themselves.

As March and Olsen note, experience seriously challenges this picture. Political institutions appear to be "more than simply mirrors of social forces." They are themselves created by past human political decisions that were in some measure discretionary, and to some degree they are alterable by future ones. They also have a kind of life of their own. They influence the self-conception of those who occupy roles defined by them in ways that can give those persons distinctively "institutional" perspectives. Hence such institutions can play a part in affecting the political behavior that reshapes them in turn—making them appropriate as units of analysis in their own right.

The role of institutions, moreover, goes well beyond providing the rules governing decision-making situations in the manner Riker stresses. It influences the relative resources and the senses of purpose and principle that political actors possess. And sometimes, at least, those purposes and principles may be better described as conceptions of duty or inherently meaningful action than as egoistic preferences. Correspondingly, the behavior they alter may serve other values than economic or systemic functionality (March and Olsen 1984, 738–42).

These criticisms can be made more concrete by placing them in the context of public-law research. It is unquestionably of some value to know that, for example, the votes of moderate justices in search-and-seizure cases are significantly correlated with facts such as the place of the seizure, the extent of the search, and the presence or absence of a warrant, and that judicial votes on civil-liberties and economic issues can to a large degree be linked to judicial attributes such as age, educational and professional background, and partisanship (Segal 1986, 942; Tate 1981, 355, 359–63). But we might learn more about the crucial factors in judicial politics by also asking if there are established police practices, or inherited values, that lead justices to think searches in certain places are more problematic; or if we identify the content and sources of the typical experiences influencing judicial attitudes that attributes like educational and professional background signal; or if we study the institutional constraints on the sorts of justices that are likely to be sitting on the bench at a given time. We might also wish to consider whether judicial voting patterns affect the types of searches police conduct; whether prevailing popular notions about the privacy of certain locations do so; whether judicial decisions on various economic and civil-liberties issues assist or alter the institutions that shape and select justices with certain attitudes; and other questions of this order.

Obviously, such issues are so complex as to be largely beyond the scope of any rigorous, essay-length study of judicial decision making. But the "new institutionalist" argument is that we must not there-

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fore displace them from the discipline's research agenda, declining even to suggest how narrower findings might have *implications* for these broader questions, not to speak of pursuing the latter directly. When limited inquiries dominate the field of research, too many of the decisive elements in politics are left unexplored. Yet in political life, many economic currents and even political actors' own purposeful commitments are affected by relatively enduring legacies of past political choices. If these elements are left permanently at the margins of analysis, the result can be a restricted, atemporal view of politics that says little about the factors it holds itself to be most potent in political life. Political action, such as judicial decision making, then inevitably seems a tedious, crassly self-interested, and rather ineffectual game among programmed players.

Adherents of rational-choice perspectives might reply that this "new institutionalism" stresses the importance of background structures and apparently inefficient historical processes *over* the intelligible free choices of identifiable political actors, thereby raising the specters of determinism, blind chaos, or both. Yet ironically, the writers who have followed the second road to the new institutionalism have done so precisely because they believe it finds more room for meaningful political decisions than the deterministic outlooks with which they began. Before further describing and assessing the implications of the new institutionalism for public law, let us trace this second road.

The Road from Historical and Sociological Determinisms

The contention that interest-group theories of politics and later rational-choice models all neglect how resources and values arise historically is, of course, not new. Numerous schools of thought endorse these accusations. The perspectives that have most influenced political science are probably Marxist historical analysis and the non-Marxist "structural-functionalist" sociology elaborated by the U.S. students of Max Weber, Talcott Parsons, and Robert Merton, as adapted by leading political scientists, notably David Easton and Gabriel Almond. Common to both these schools is an effort to go beyond the surface of individual and group conduct to identification and analysis of the deeper social and economic forces that—interest-group analysts acknowledge—seem to drive human political life. Marxist and non-Marxist sociologies identify these forces differently, however.

In its most bare-bones versions, Marxist historicism provides the famous argument that relationships to the means of production are the root determinants of the prevailing modes of social and political organization and the processes of conflict and change they display. Political actors can be placed into classes according to where they stand in relation to the dominant productive forms, and their conduct (and that of the institutions they create to further their interests) will ultimately be explicable in terms of the economically based imperatives facing those thus situated (Tucker 1978, 473–83, 487).

In contrast, Parsonian "systems analysis" or "structural-functionalism" refuses to give such utter preeminence to economic relations. It argues instead that human life should be analyzed in terms of the multiple, interlocking social, cultural, and psychic "systems" that organize activities in ways that prove to be more or less enduring. On this view, all systems with any prospect of survival share certain formal characteristics and behaviors, various functional necessities and modes

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of adaptation to meet those necessities. Among these are certainly functions of resource production but also functions of socialization (or "pattern maintenance"), coordination (or "integration"), and "goal attainment" more generally. While studies of individual and group behavior can be included within such a framework, these types of systemic structures and functional needs become the focal points of sociological analysis. Within political science, David Easton (1953) influentially modified Parsonian systems theory by laying greater stress on the distinctive importance of the "political system" and on "input-output" analysis, while Gabriel Almond (1960) emphasized the adaptive features of systems in order to elaborate a less static, more developmental version of structural-functionalism. But all these writers shared a concern to provide theories that would explain behavior in terms of the needs of deeper social structures without deriving all behavior from the prevailing mechanisms of economic production (Barbrook 1975, 40–67; Parsons 1951; Skocpol 1984, 2–4).

Despite that difference, recent scholars with historical, sociological, and often Marxist sympathies have rejected both these older Marxist and non-Marxist alternatives to individual and group behavioral analysis. Again their criticisms are many, but the basic objection here is that these positions go too far, treating history, politics, ideologies, and institutional behavior as *entirely* dictated by economic or systemic necessities. Instead, many contemporary writers, like March and Olsen, stress the "relative autonomy" of at least some aspects of political action from such deeper structures, and they perceive many fairly long-lived social systems and processes of historical change as tragically inefficient, nonfunctional, indeed oppressive.

Different scholars make these points in different ways and to different degrees. Within political sociology and political science, Theda Skocpol (1979) and Stephen Skowronek (1982) have provided influential analyses of how *state* institutions have played a relatively independent role in shaping political development during the French, Russian, and Chinese revolutions in Skocpol's account and during the more peaceful and piecemeal institutional adaptation to industrialization in the United States described by Skowronek. Inspired in part by Antonio Gramsci, many historically minded members of the Conference on Critical Legal Studies have instead emphasized how legal ideologies, like political ideologies conceived more broadly, are "relatively autonomous" from economic and systemic "necessities." Hence, legal doctrines merit careful study. In the words of Mark Tushnet (1981, 30–31), they form "groupings of ideas connected by repeated association," which may be burdened by "internal tensions." Those tensions can generate some processes of adjustment and change that are largely independent of outside forces. Any adjustments made may in turn shape the choices and behavior of, at least, legal actors within the political system. People, it is assumed, are psychically driven to "interpret the material conditions of their existence in ways that make their experience coherent," and they may at times seek to alter those conditions in order to make coherent interpretations more possible.

Critical legal scholars disagree on just how "autonomous" legal ideologies are. Tushnet (1981, 30) remains close to traditional Marxist perspectives by arguing that "we can still expect the law to embrace positions that are required by the interests of the ruling class as a whole, even if they are inconsistent with the interests of individual members of that class. The law remains linked to the relations of production directly through the political perception of advanced segments

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of the ruling class and indirectly through the political principles that are ultimately rooted in those relations."

But Robert Gordon (1984, 101) breaks more fully with Marxism, as indicated by his influential essay, "Critical Legal Histories," which attacks both Marxist and Parsonian forms of "evolutionary functionalism" in legal history precisely as contemporary political scientists and sociologists attack its counterparts in their disciplines. Gordon insists that "causal relations between changes in legal and social forms" are "radically underdetermined." Legal forms and practices therefore cannot be adequately grasped as "objective" responses to "objective" historical processes, not even evolving relations of production. Law is indeed a "product of political conflict," but it is not simply a mirror or reflex of that conflict. Again, legal ideologies are "relatively autonomous" structures with their own peculiar internal character, so that they sometimes act as "independent variables" that transcend and actually help "shape the content of the immediate self-interest of social groups." Judges may decide in part out of concern to mitigate internal tensions in legal doctrines; parties to a case may be influenced by what the law suggests their legitimate claims are. Partly as a result, there can be no confident expectation that the decisions of legal actors or institutions will always be "functional" for their material interests, or even for their own survival: the quest for ideological consistency can lead to behavior that is counterproductive by these standards. Thus these legal and sociological critics of functionalist accounts arrive, like March and Olsen, at a belief in the importance of various relatively enduring political and intellectual institutions in human life beyond economic relations and social "systems"; but they do so as much to break free of rigid historical determinisms as to identify overlooked constraints on political choices.

The Lessons for Public Law: An Appraisal of the "New Institutionalism"

Strengths

While not every protest against dominant perspectives deserves to be sustained, it is not likely that many scholars would feel obliged to move in the same direction unless there were something to their feelings. I believe the basic critiques advanced by those on each road to the new institutionalism are correct, whether or not they really point toward that destination. Those reacting to individual and group behavioralism and rational-choice theory are right to insist that analyses of politics should explore how relatively enduring structures of human conduct have shaped the existing array of resources, rules, and values, instead of simply taking that array as given.

As just suggested, this point is strikingly supported by the study of public law—for controversial, politically charged judicial decisions in the past may later determine the types of litigants that can get *into* court at all, as well as the very types of claims or rights persons believe they are entitled to assert, morally as well as legally (Orren 1976; Gordon 1984, 109–13). Obviously, no group is likely greatly to influence an institution that will not attend to its voice. And many groups' sense of their nature and purposes may be significantly affected by how far the legal system legitimates, for example, the permissibility of religious dissent, or of unregulated production and exchange. Thus, accounts of self-interested rational calculations and the behavioral regularities they are thought to generate will have limited explanatory power if they are not sensitive to how the legacies of past decisions lead people to think their interests should be so defined. Neglect of these factors may also prevent us from seeing how social definitions of interests appear much

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less rational, and much more vulnerable to alteration over time, when their origins are identified.

Those reacting against deterministic historical sociologies also have a strong, if less decisive, case. As even admirers of Marx and Parsons have felt compelled to conclude, the history of human societies simply cannot be adequately captured by the paradigms of economic or functional necessity alone. Again, for anyone immersed in public-law materials it seems all too evident that judicial decision makers often have a significant range of choice on how to advance the interests and principles they perceive as imperative. And while they always hope to reach results that will be beneficial at least for some, they often fail to decide in ways that serve the interests of the ruling class, of economic development, or of systemic adaptation. It is difficult to see much that is functional in Roger Taney's *Dred Scott* opinion, for example; and while it tried rather ineptly to assist the southern white ruling class, it was hardly cheering news for U.S. capitalists (Dred Scott v. Sandford, 19 Howard 393 [1857]; Fehrenbacher 1978, 340–414, 551–61). Many modern decisions, like the courts' much decried insistence on stopping a nearly completed dam to save the snail darter in *Tennessee Valley Authority v. Hill* (437 U.S. 153 [1978]), again obviously please certain groups, but they do not so clearly further either dominant economic interests or systemic efficiency (Dworkin 1986, 20–23).

If neither accounts of rationally self-interested behavior nor the leading deterministic sociologies seem adequate to describe political life, it indeed seems wise to identify other relatively lasting structures or patterns of behavior, institutions of various kinds, that shape and constrain political choices and conflicts. The alternative would be to fall into a simple recitation of events, an approach favored by some journalists and historical purists, but one that cannot shed much light on deeper regularities or causes in human affairs. Various political scientists and sociologists have noted that "new institutionalist" approaches instead resemble the sort of history advocated by Fernand Braudel (1980) in his influential chapter, "History and the Social Sciences: The *longue durée*" (Skocpol 1984, xii, 394; Mayhew 1986, 9). Braudel called for students of human affairs to turn from their concentration on the immediate, surface politics of kings, generals, and dramatic events to elements that operate over longer time spans, to middle- and long-term patterns and "structures" that condition those actions. As examples of this sort of history and social science, studies of the *longue durée*, Braudel (1980, 31) mentioned "geographical frameworks, certain biological realities, certain limits of productivity," and even "spiritual constraints: mental frameworks too can form prisons of the *longue durée*." This list, it should be stressed, defines the historian's task as significantly broader than that posed by new institutionalists in political science and sociology. They typically focus on humanly created structures—including "normative orders"—so they consider only a portion of Braudel's "middle-term" factors (March and Olsen 1984, 243–44). Even so, the methodology Braudel describes is indeed parallel.

Problems

But the similarity of the "new institutionalism" to Braudel's history suggests two basic difficulties the approach faces, at least as a model for public-law scholarship. The first problem is one sometimes held to characterize the work of many followers of Braudel, and it returns us to a point raised by the two contrasting roads to the new institutionalism: the question of whether these analyses *increase* or *decrease* our sense of the discretionary nature and the significance of political action. Histories focused on the *longue*

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durée are sometimes said to treat the individual conduct featured in traditional political and legal histories as unimportant—once again, epiphenomenal. Perhaps that is the truth about such conduct; but if it is, then while the new institutionalism would restore the study of a wider range of specifically political structures, it would still downgrade the significance of the choices of human political actors. We would, for example, place little stress on the existence and importance of genuine judicial discretion in decision making—the phenomenon that is traditionally at the heart of public-law analyses, whatever their methodological differences. Instead, we would tend to speak simply of "institutions" and "structures" acting in the legal world, in a way that many find far too full of reification and anthropomorphism to be plausible (Easton 1981, 316).

While some "new institutionalist" writings do seem to neglect the significance of individual choice (particularly in favor of "the state"), there is no need for the approach to be taken so far. The new institutionalism requires us only to stress how background structures *shape* values and interests, not to speak as if they have interests of their own. Most of our experience certainly suggests that human actions such as judicial decisions are indeed influenced by a great range of structural contexts—by the actor's position within state agencies or political parties, by economic relations, by ideological outlooks, by enduring ethnic alliances, and so on. But the result is often that actors are faced with so many conflicting imperatives that they retain significant room for choice, even if many of their alternatives are fairly grim.

I think in fact that if the study of politics, including the politics of law, is to seem worth pursuing as a distinctive discipline, it must begin by assuming that what have traditionally been thought of as political actions *can* be independently important forces in reshaping the world—though of course its investigations must bear this assumption out. As Riker has argued, in searching for patterns and regularities, we certainly must attend to "structural and cultural constraints" that act as "unstable constants" to help us make political actions somewhat more comprehensible and even predictable. But our accounts should at a minimum leave open the possibility that the outcomes of decisions, which may significantly alter background structures and constraints, are in part traceable to the creative political skill, judgment, and artistry of the actors involved, as well as to the forces that have shaped them and the situations they confront (Riker 1980, 445).

As suggested above, Riker's rational-choice approach falters in large part because it does not take this last point far enough: it fails to recognize that the very values of political actors may be altered by deliberate reflection and choice, as well as by the mutable political institutions that shape the formation of values and beliefs more broadly. While such alterations may be rare, they should not therefore be dismissed as trivial "outliers," for they may be among the most decisive of political events. In this regard, it is useful to note one feature of the legal perspectives that have been most skeptical about the power of past legal doctrines to determine decisions, the legal realism of the 1930s and the critical legal scholarship of today. When they proceed to describe judicial decision making in ways free of "legalistic" biases, such writers end up attributing to "strong" judges significant creative capacities, both to alter the messages conveyed by legal institutions and to transform their own inherited beliefs over time. These "strong" judges are often credited with pivotal, lasting changes in the direction of the law's evolution (Carter 1985; Llewellyn 1960, 70–74).

That point brings us, however, to the second and more basic problem of these

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recent approaches. If we emphasize the "relative autonomy" of political actions from any particular structural determinant or array of determinants and if we also assume that political actions can play a significant role in altering many of these structures, or "unstable constants," it follows that most of these background structures should generally be treated as not wholly reducible to others. Apart from any other factor, the intervention of unpredictable human acts will probably lead each institution that is vulnerable to such acts to develop distinctively. Thus each will display some "relative autonomy" of its own: state agencies will be influenced by, but still relatively independent of, economic forces, and vice versa; political and religious ideologies will bear a similar relation to legal institutions; family structures will be similarly related to such belief systems, and so on. In short, as we travel these roads, "relative autonomy" soon becomes ubiquitous. Our picture of politics is expanded to include the whole man-made world, filled with multiple structures and political actions, all mutually influential, but none simply expressions of any others.

And that picture, of course, may seem so hopelessly complex and foggy as to be impossible to bring into focus. Everything is somehow connected to everything else, but we seem to have little purchase on what structures are more or less important and to what degree. Such an account may be better than very particularistic narrative histories that make no effort to discern enduring structures, patterns, and behavioral regularities at all; but it seems incapable of approaching the rigor and precision to which political scientists aspire. In public law, for example, we might have only slightly modified accounts of how enduring "structures" of interest groups place recurring demands on courts, of how dominant coalitions and realignments affect judicial behavior, along with qualitative probings of legal ideology and major decisions and quantitative studies of how judicial attributes correlate with results. But it is not clear how we would tell which accounts were more decisive or whether the others really mattered at all. In that case the "new institutionalism" would simply provide a convenient rationale for continued eclecticism, much as some say "political jurisprudence" turned out to be.

Suggestions

That outcome may be unavoidable. But the reasons for moving in this direction appear compelling, so perhaps we should try to see how these difficulties might be overcome. It is premature to advance settled answers, but some observations are possible. The fundamental methodological question is how students of politics, and in particular of public law, can give greater specificity to the precept that the "interplay of meaningful acts and structural contexts" should be central to their analyses. Four steps, relatively obvious but rarely fully implemented, suggest themselves.

Plainly, the first step would be for public-law scholars of various stripes explicitly to conceive of their "independent variables" as relatively enduring structures of the sort described here. Analysts oriented toward the study of interest groups might, for example, wish to study an area like labor law by exploring the impact of recurring *relations*—or patterns of conflict—between certain employer and employee groups. Students of legal ideology might instead take certain ongoing elements of "liberal legal" thought on property rights as their starting points for analyzing those same labor decisions. While each set of scholars would be building on existing approaches, significant modifications would be involved. Interest-group analysts would, for example, focus on discovering empirically ascertainable and important his-

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torical regularities in group interaction rather than simply identifying instances of patterns predicted by formal theory. Students of legal ideas would have to present more explicit models of the key mental or rhetorical structures they wished to explore than is often the case.

Secondly, analysts would generally be expected to provide some indication of the origins of the structures or institutions they examine, with particular sensitivity to how those structures may have arisen from past, controversial political choices. Obviously, a full explanation of a political institution's genesis would normally require a separate study. But if scholars provide at least some indication of the sources of the structures they deal with, both they and their readers will be less likely to view those structures, and the types of political life they shape, as patterns of behavior writ into the nature of things. They are thus more likely to be attentive to possibilities omitted in the prevailing order of political life, and to developments within it that may alter the institutions that have helped give rise to it over time.

Third, to execute this approach adequately, all analysts (and particularly those engaged in qualitative studies of patterns in thought and argumentation) should identify as fully as possible their dependent variables, the set of "meaningful acts," such as judicial decisions, they claim to help explain. It is true that qualitative analyses face some special burdens in this regard. If one is trying to ascertain the presence and saliency of not just certain catchphrases or broad normative dispositions but specific types of reasoning about, for example, property rights, then it is hard to codify those patterns into a data set suitable for statistical manipulation. A qualitative, interpretive narrative that shows the structures of thought and argument to be visible in texts is required. The extensiveness of such narratives alone can incline a writer to focus on a few major cases that seem representative or determinative of most judicial actions in a given area, instead of documenting how those structures are visible in all or most of the relevant cases (see, e.g., McCloskey 1960).

That sort of narrative can be conducted in compelling fashion, and so it may be a reasonable methodological choice. Yet if qualitative analysts define certain patterns of thought and postulate their likely influence with much rigor at all, it does not seem too much to ask them at least to indicate the relevant universe of cases and decisions where such patterns are held to be visible, and the portion of those cases in which they believe their claims are borne out. And if a scholar chooses to explore only a few leading cases in depth, it is reasonable to require some justification for granting those cases that leading status.

On the other hand, the approach described here would urge both quantitative and qualitative analysts to consider not only how far actual decisions conform to the results their structural contexts lead them to expect but also how those structures may have led decision makers like judges to perceive their situations and opportunities in ways that seem quite wrongheaded from other perspectives. In this regard, qualitative work on "legal consciousness" has generally been much more probing than quantitative studies (e.g., Klare 1978). Both qualitative and quantitative scholars can be criticized, moreover, for stressing only how their independent variables, here described as enduring structures of behavior or institutions, explain much of the variance of actual decisions. As I have argued, an adequate account must also acknowledge that some decisions appear to reflect judicial creativity, in whole or in part, in ways that may be comprehensible in light of broader factors but that are not reducible to them.

Finally, public-law studies of the inter-

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relations between legal choices and the "background" institutions that shape them should at least raise questions about how those choices have in turn affected such institutions, intentionally and unintentionally. Again, no manageable analysis can be expected to explore these ramifications in depth; but they must at least be suggested if we are to approach the broader political significance of the decisions analyzed. We must not only try to explain *Dred Scott;* we must also consider what developments *Dred Scott* helps to explain.

These points describe the basic alterations in existing forms of research that would make them expressive of a common focus on the dialectical interplay of meaningful decisions and structural constraints. This approach might succeed in making judicial choices explicable in terms of relatively constant structural factors, avoiding the errors of assuming that all can be explained in terms of individual or group calculations. At the same time, it would preserve the possibility that the actions themselves may not prove epiphenomenal to any combination of background factors, and that they may have unexpected significance for later events. Those features would save the approach from the pitfalls of both historical determinism and the undue reification of "structures."

Once analyses were thus recast, moreover, I think public-law scholars would find themselves in a position to seek further kinds of interconnections, both between different modes of descriptive work and between empirical and normative concerns. The quest for such interconnections is vital to the larger descriptive enterprise of the subfield, for clearly no scholar can hope to deal with more than a fraction of the possible structures influencing the legal actions he or she explores. But if many analysts first cast their work as explorations of the interactions between diverse specified structures and sets of decisions, they could relate them more readily to parallel studies in the field. Indeed, an individual scholar might wish to explore how far two disparate structures—such as, perhaps, the Jeffersonian political coalition dominant in the executive and legislative branches, compared with persisting laissez-faire economic doctrines—influenced judicial reasoning on the commerce clause in the first quarter of the nineteenth century. The analyst might generate different predictive hypotheses based on these different structures and then determine which ones were best borne out, while attending in conclusion to the consequences of the decisions for existing political coalitions and economic ideologies. Students in the field could then gain some sense of the comparative explanatory power of these different structural contexts, as well as the ways actual decisions transformed them.

Of course, there will often be significant interactions in the impact of different background structures. In the case of quantifiable factors, contemporary techniques of interactive computing in multiple regression analyses are increasingly able to model some of these relationships as well as the relative impact of different variables. Here, however, is where the differences between quantitative and qualitative analyses become most troubling. If we have, for example, a model of Supreme Court decision making based on the place of the litigants in prevailing economic or political power structures that proves to correlate reasonably well with the actual pattern of decisions in voting-rights cases, how can we connect it precisely to a less quantifiable account of prevailing conceptions of representation and their problems that claims to capture the logical tensions and patterns of argument in those cases? No perfect fusion of these modes of analysis can be expected, but some points of contact are possible.

The two types of models might to some degree be properly viewed as comple-

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mentary, shedding light on different aspects of behavior, one on the causal determinants of the vote, the other on the patterns of discourse used to rationalize the vote. But if legal discourse truly displays some "relative autonomy," then it will at times play more than this justificatory role. By identifying cases where conceptual analysis would suggest different voting behavior than the litigant model, we could ascertain whether this "relative autonomy" is indeed manifested. And by seeing which model accounts for the most decisions in cases where their predictions conflict, we could still get at least a rough ordinal sense of the relative explanatory power of the two analyses. If the conceptual account appears to have some independent validity, moreover, we might be able to get some indication of how it interacts with other factors by seeing if its results correlate better with some quantitative models than others.

It is true that because of the differences between qualitative and quantitative analyses, these comparisons and correlations will probably remain rough at best. We will be able to say, for example, that more cases are explained by a "realignment" explanation than by a particular conceptual model or that the latter correlates more clearly with a judicial "party origins" analysis than one stressing "socioeconomic origins." But precise cardinal estimations of just how much variance each model explains, by itself or in interaction with others, are not likely to be convincing. Even relatively crude estimates, however, might enable us to address some important questions that are not specifically on many current research agendas.

In particular, it might be possible over time to build up a fairly rich body of theory concerning *when* certain sorts of structural contexts are likely to be more or less relevant, drawing implications for broader patterns of institutional evolution. For example, economically dominated decision making of the sort that preoccupies Judge Posner's "law and economics" school might prove to capture regularities of choice well in certain contexts (Posner 1977). But the processes leading to the prevalence of those economic concerns—and, indeed, efficacious economic decisions themselves—may eventually alter the situation in certain fairly predictable ways. Narrowly economic reasoning might prove to be most visible when utilitarian patterns of thought are prominent and economic crises evident, as in, perhaps, the 1890s, the 1930s, or the 1970s. Once decision makers respond to those pressing concerns, however, less directly economic constituencies, claims, and structures of thought may come to the fore. If so, then wealth-maximizing models of decision making would properly find their place within a broader theory that explains when such patterns of reasoning, as opposed to other enduring patterns of behavior, are likely to be most salient. As March and Olsen note (1984, 742), it is such broader theory that the "new institutionalism" thus far lacks.

And significant theoretical progress along these lines may, of course, prove unattainable. But in any case, if these directions are pursued, we would not regard scholars who try to see how far ideological structures shape judicial decisions and scholars who analyze the impact of party realignments or appointive processes as engaged in sharply opposed enterprises. That sense of a common endeavor might make qualitative scholars more aware of the need to connect their claims with measured patterns in actual decision making, and it might also promote quantitative studies more sensitive to the complex conceptual structures and characteristics of political beliefs.

Furthermore, one even more significant consequence might result. The agenda of empirical investigations might become

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better tailored to enrich the normative debates in the field, and those debates might be more attentive to the problems of the empirical generalizations they rely upon tacitly. To see how, we must identify further the recent philosophic developments that suggest the relevance of "new institutionalist" descriptions of the legal system.

The New Institutionalism and Normative Debates

After a period in which rather abstract and hypothetically oriented moral and political theory was in vogue, many philosophers have recently been urging the adoption of more concrete, particularistic, and historically infused modes of moral and political argument. Writers who describe themselves as subscribing to quite different substantive outlooks, including neo-Aristotelianism, Enlightenment liberalism, and democratic socialism, have agreed with Alasdaire MacIntyre (1981, 119) that "morality is always to some degree tied to the socially local and particular." Therefore, as Richard Rorty (1986, 13) argues, following John Dewey, moral philosophy is properly concerned with the "precious values embedded in social traditions," and with the "conflict of inherited institutions with incompatible contemporary tendencies." Consequently, we are now frequently urged to begin not with states of nature, past, future, or hypothetical, but "with the values we hold and the political world we inhabit," examining the record of the diverse inherited values in our past and present social existences in order to deliberate on what prospective courses seem more or less promising (Herzog 1985, 225).

This recent turn (or return) in moral and political philosophy, which may be called a move away from "ideal theory" to more empirically oriented "pragmatist" approaches, seems to me largely sound. It does raise legitimate fears that we will be too wedded to past values or else inclined to doubt our values if we see them as merely our own. Pragmatism must indeed prove unsatisfactory if we use it to conceal from ourselves the necessity to criticize the current array of normative dispositions and to make reasoned choices among them. Those choices may have to be defended in turn by appeals to what seem to us to be more lasting aspects of our condition, in the manner of natural-law theorists.

Yet it simply seems true that our moral outlooks are largely the products of past traditions, whether we are conscious of that fact or not. So we are more likely to be liberated from the inadequacies of our inherited principles if we recognize their historical roots and attend carefully to the role—great or small—they have played politically, attempting to judge their characteristic tendencies, strengths, and weaknesses in the crucible of social life. Furthermore, recognition that our values have been carried to us via particular traditions does not preclude faith in their transcendental validity, much less their propriety for us. And if it makes us somewhat more dubious about making universalistic claims, generally that may be to the good.

It is fair to say, however, that so far the call to begin with the empirical and historical realities of our moral traditions remains more a program than an achievement. While Rorty, MacIntyre, Walzer and others have offered useful historical analyses of certain grand philosophic traditions, few of these writers have paid any detailed attention to the other varieties of political and moral discourse that have played such a prominent part in shaping our current societies and selves. Clearly, if the "new institutionalism" is understood in the manner proposed here, as encompassing among other things descriptions of the influence of enduring

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structures of legal ideas, it is well suited to help fill this gap. Research that identifies actual patterns in legal and political discourse and their consequences, testing their significance versus that of other structural contexts, should enable public-law scholars to argue more powerfully about the values U.S. law has really embodied historically, about the ways those values have shaped, and been shaped by, political conflicts, and about the results they have furthered or forestalled.

Those studies would be of great relevance for many contemporary normative debates, such as, for example, whether more "republican" or "liberal" conceptions of civic life have been dominant in U.S. public acts in particular eras, and whether "republican" conceptions have most assisted democratic reforms or reactionary localism (see, e.g., Bellah et al. 1985). Often, I fear, theorists would have to acknowledge that the historic traditions they favor, whether "republican" or "liberal" or both, have often been relatively ineffective in furthering the ends they admire, and have even proven conducive to quite unattractive results. If so, many current normative arguments would be greatly altered by such increased historical awareness. And since I believe debates over the values that should guide the U.S. legal system in the future must always remain a prominent part of the public-law agenda, this contribution of "new institutionalist" studies of historical ideological structures is a vital one indeed.

At the same time, analysts engaged in more quantitative empirical research might benefit from an increased awareness of how their studies are relevant to ongoing normative controversies. When a quantitative scholar can see quite easily how prevalent moral disputes rest in part on empirical claims about the historical operations of the legal system, he or she is more likely to choose to research topics that speak sharply to those disputes. For example, much of the current discussion of "law and economics" takes place on an abstract normative level. Scholars ask, Is it desirable to think of the legal system's goals in this way? But there are attendant empirical questions. For instance, Is it adequately realistic, or historically plausible? To convince us that it is, Posner, among others, has advanced strong claims that much of nineteenth-century common law is explicable in terms of economic reasoning (Posner 1977, 13–14, 18; Posner 1981, 5, 106, 114). Those claims are susceptible to much fuller empirical and historical investigation than they have heretofore received from either the advocates or critics of this viewpoint. (Even when adherents of this school do not claim that economic reasoning was consciously employed, they generally assert that their models capture the "deep structure" of common-law decision making, a claim that is empirical on its face and that is in any case dependent on arguments about what actually was economically efficient in the historical periods in question.) The results of such investigations are likely to affect the credibility of such economic reasoning as a normative model for contemporary jurisprudence. More generally, a public-law subfield that explicitly took part of its empirical agenda from normative debates and that produced descriptions relevant to current normative claims would go far toward advancing the unity of descriptive and prescriptive concerns that some participants in the 1984 symposium found lacking.

All this may be too optimistic. At present we can be more confident of the critiques of current efforts than of the potential of the suggestions offered here. But I would insist that at a minimum, these recent trends mean first, that empirical investigators should agree that the impact of structures of ideas forms a part of their enterprise; second, that qualitative analysts of the history of legal ideas and

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normative advocates should recognize that their endeavors will seem too abstract if they do not study carefully the role that the values they appeal to have played in the actual judicial decisions that constitute so much of our legal traditions; and, I would hope, third, that both quantitative and qualitative public-law scholars might consider exploring further how we might realize this suggested recasting of their work in terms of a common focus on the interplay of specified structures and decisions. If the methods and conceptions of the "new institutionalism" can help promote such shared awareness among the diverse scholars at work in the field, these recent developments will indeed prove to have both importance and promise for students of public law, as well as for political scientists in general.

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Rogers M. Smith is Associate Professor of Political Science, Yale University, New Haven, CT 06520-3532.

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INTEREST GROUP POLITICS AND ECONOMIC GROWTH IN THE U.S. STATES

VIRGINIA GRAY
University of Minnesota
DAVID LOWERY
University of North Carolina
Chapel Hill

Mancur Olson's model of economic growth has attracted great attention as a theoretical account of how interest groups influence the rate of economic growth over time. Moreover, the model appears to have received strong empirical support in Olson's tests employing U.S. state data. However, the specification of the Olson construct in these tests is insufficiently attentive to the complex causal chain implicit in Olson's argument, inadequately accounts for precisely how interest groups matter, and employs a static research design that obfuscates the cause-and-effect relationships posited by the model. We review these issues and develop a more complete specification of the Olson model. The respecified model is then tested using U.S. state data for the period of the late 1970s and early 1980s using new measures of interest-group influence.

Mancur Olson has provided a compelling and broad ranging theoretical account of the manner in which interest groups influence economic growth rates. Building on his analysis of the mechanics of interest-group organization in *The Logic of Collective Action* (1965), he suggests in *The Rise and Decline of Nations* (1982) that over time, the number and influence of groups interested in retarding economic growth accumulate and are incrementally successful in having antigrowth policies adopted and implemented. Unfortunately, one of the long-term consequences of such policy adoptions is a decline in economic growth rates as nations move temporally further from the periods of invasion or domestic upheaval that serve to upset patterns of interest-group organization and influence. The Olson construct has, not surprisingly, attracted a great deal of theoretical comment (Mueller 1983; Willis 1986) and has been the subject of some degree of empirical evaluation (Brace 1985; Brace and Dudley 1985; Choi 1979; Dye 1980). Olson (1982) himself tested his model on both cross-national and U.S. state data in a truly comparative study. In this paper, we confine our attention to the Olson model as applied to the 50 U.S. states.

While much of the U.S. empirical evidence supports Olson's model, it remains suspect for several reasons. First, the specification of the Olson construct upon which this evidence is based is at best incomplete; little attention was given by Olson to specifying the complex set of relationships that lie between the initial observation of time since invasion or upheaval and changes in the rate of economic growth. Second, the specification of the precise manner in which interest-group power is influenced by the passage of time and, in turn, influences policies affecting growth rates is ignored in Olson's tests of the construct. Instead, Olson and Choi simply examined time

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and growth rates, something that is surprising given that the model is a theory *about* interest groups. And third, most tests based on such inadequate specifications of the Olson construct have examined time periods that are too long to allow for valid inferences about cause and effect and only address indirectly "changes" in the concepts identified in the model. In short, most existing tests of the Olson construct are inadequate. Recent research by Brace and his associates (Brace 1985; Brace and Bauman 1986; Brace and Dudley 1985) is the chief exception.

We develop and test a more complete specification of the Olson model. The model is reviewed in the first section, where we develop a series of equations that are more consistent with the complex set of relationships posited by Olson. The model is then partially tested in the second section with U.S. state data for the period of the late 1970s and early 1980s. The test results are discussed in the final section.

Evaluating the Theoretical and Empirical Foundations of the Olson Model

Extant theoretical specifications and empirical evaluations of the Olson hypothesis are characterized by at least three major problems. These problems concern, respectively, (1) the theoretical specification of the causal chain underlying the model, (2) the specification of just how interest groups matter, and (3) the appropriate research design to assess validly the cause-and-effect relationships posited by the construct.

The Problem of Specifying the Causal Chain

Olson's hypothesis appears deceptively simple. As Olson (1982, 77) has stated it, "The logic of the argument implies that countries [or states] that have had democratic freedom of organization without upheaval or invasion the longest will suffer the most from growth-repressing organizations and combinations." In this form, the model has only two variables: time since invasion or upheaval and the rate of economic growth. And this is certainly the form of the argument that has received the most empirical scrutiny. The empirical tests of Olson (1982, 94–117), Dye (1980, 1099–1100), and Choi (1979) all use U.S. state data on the relationship between time since admission to the Union (or since the Civil War for southern states) and economic growth rates. In this usual form, then, Olson's model is tested with the following equation:

$$\text{GROWTH}_i = a_1 + b_1\text{TIME}_i + e_i \qquad (1)$$

where GROWTH is the economic growth rate and TIME is time since major domestic upheaval or war. Moreover, these tests have generally provided very strong support for this specification of the Olson model, with the estimated coefficients being negative and significant as expected.

But while the model starts with time and ends with the hypothesized growth effects, there are several other causal steps that must necessarily come between, linkages that remain untested with the specification presented above. These untested linkages between the two variables cited in Olson and Choi's presentations and tests are at least implicit, and in several cases quite explicit, in more complete presentations of the model. Specifically, four steps are implied in the hypothesized relationship between time and growth.

First, length of time since major upheaval or invasion is hypothesized to be associated with growth in the "number" of groups organized to pursue particularized benefits for their membership. As we will see later, time may also be related to other characteristics of interest-group

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influence. And while most of the empirical tests of the Olson model have focused on the more indirect relationship between time and growth rates, there has been some attention to this more specific hypothesis in evidence linking the "size" of the unionized population to time since statehood (Dye 1980, 1103–4; Olson 1982, 106–7). However, this evidence is at best indirect since the Olson hypothesis speaks to *number* of groups in the political system, while the test addresses the membership *size* of one kind of group. Moreover, even this relatively limited effort to assess a key linkage of the model empirically is missing when the other linkages are considered.

The *second* of the missing linkages can be expressed in the hypothesis that the number of interest groups is associated with the adoption of policies designed to minimize the vulnerability of the groups' members to the consequences of market competition. Such actions usually involve the use of government for private economic benefit, either through passive acquiescence by government to private collusive arrangements or through more active regulations to restrict entry, fix wages and prices, and generally minimize the burden of competition. This link in the Olson construct is the one that is most clearly political, addressing both the kinds of policies groups will pursue as well as their ability to realize such goals via influencing governmental decision makers.

But while the logic of this relationship is fully developed in the economic literature on regulation (e.g., Pelzman 1975; Stigler 1971), there has not been enough effort to confront the hypothesis with data. Indeed, in Olson's presentation of the model, these effects are merely listed (1982, 44–45), with little effort to link their adoption and use to the growing number of organized groups. In one of the few studies to assess the relationship between interest-group strength and such policy adoptions directly, Ambrosius (1985, 12) did find evidence that states with "strong interest groups are more likely to attempt to ensure or improve their economic well-being by adopting economic development policies." But even this evidence must be viewed as tentative, given the scope of the interest-group strength measure used in the study; Ambrosius's measure included a large number of groups, many of which would seem at first glance to have little interest in such policies. In an earlier study, Dye (1980) found that aside from highway spending, no other policy variables independently affected growth rates. Growth in the number of groups must be linked to the adoption of specific policies or practices if Olson's model is to be validated.

Third, the Olson model implies that adoption of these policies is associated with changes in the patterns of economic behavior associated with investment, innovation, and adaptation. That is, not only must it be shown that the number of interest groups is associated with the adoption of particular policies, it must be shown that these policies influence the way economic actors conduct their activities. Quite simply, economic growth rates are merely the outcome of a complex set of economic behaviors. The success of this policy-behavior link is fully implied by the Olson model. But while all the evidence is far from in, it has long been argued that the kinds of policies adopted by states under the heading of industrial policy probably have little impact on growth and may in fact be detrimental over the long run (Ambrosius 1986; Barker 1983; Hansen 1984, 1986). The Olson model, however, only assumes that this is the case.

Fourth, changes in these economic behaviors are hypothesized to be associated with changes in the ultimate dependent variable: the rate of economic growth. That is, it is these changes in investment,

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innovation, and adaptation behaviors presumably induced by the adoption of specific policies that eventually influence the rate of economic growth.

Thus, far from being the simple relationship specified in Equation 1, Olson's model is more correctly specified in the following manner:

$$\text{INTERESTS}_i = a_2 + b_2\text{TIME}_i + e_i \quad (2)$$

$$\text{POLICIES}_i = a_3 + b_3\text{INTERESTS}_i + e_i \quad (3)$$

$$\text{BEHAVIORS}_i = a_4 + b_4\text{POLICIES}_i + e_i \quad (4)$$

$$\text{GROWTH}_i = a_5 + b_5\text{BEHAVIORS}_i + e_i, \quad (5)$$

where in addition to TIME and GROWTH, INTERESTS is the number of groups concerned with particularized benefits, POLICIES is the level of reliance on policies designed to protect group members from the effects of economic competition, and BEHAVIORS is the level of particular kinds of economic behaviors hypothesized to influence the rate of economic growth. All of the coefficients but b_5 are expected to be positive.

Importantly, these effects are interdependent. That is, rejection of any one of these linkages would seriously weaken the chain of logic upon which Olson's construct rests. While we cannot say that failure to generate support for any one of the equations is entirely sufficient to invalidate the construct as a whole, the path outlined by this model is probably the most direct and plausible in an era when government is deeply involved in private economic relationships. Thus, if any one of the links is rejected, the plausibility of the model as a whole must decline. For instance, if little support is found for Equation 3, it must then be shown that institutional sclerosis is entirely generated outside the governmental-policy process via private labor-management accommodations. Such a path would seem unlikely. Thus, failure to support Equation 3 would seriously weaken the construct as a whole even if empirical support for Equations 2, 4, and 5 were found; simple relationships posited by those equations might still tell us something useful about time, interest groups, and economic growth; but they would not add up to the Olson construct.

While a test of this more complete specification is essential to evaluate the model fully, we have seen that most research attention has focused on the simple relationship between time and economic growth. This approach ignores the important interest-group linkages in Equations 2 and 3, something that is surprising given that the Olson model is preeminently a theory about interest groups. Yet only Brace (1985) addresses interest groups directly, using the Morehouse index of pressure-group strength. Otherwise, the only indirect evidence on the U.S states consists of Choi 1979, Dye 1980, and Olson's (1982) findings on the growth of unionization over time, an effect that may or may not be related to the hypothesized increase in the number of groups.

The Problem of Specifying the Interest-Group Effect

The second problem with the Olson construct concerns the precise way in which interest groups are expected both to be influenced by the passage of time and to determine the adoption of specific policies. As presented by Olson, the model addresses only the *number* of interest groups; and even this dimension is often inappropriately measured as the *size* of a single interest group's—the unions'—membership. Yet, Olson's construct has much more to say about the nature of interest groups than simply the number operative at any point in time. What characteristics of groups, then, should the model be attentive to? And more importantly, how are each of these interest-

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group effects related to both the passage of time (Equation 2) and the adoption of policies designed to protect the members of the groups from the forces of economic competition (Equation 3)?

Olson's presentation of the model points to three characteristics of interest groups that are hypothesized to be associated ultimately with economic growth. The *first*, as noted above, is the simple number of organized interest groups in the society. As Olson (1982, 17) states it, "Developed democratic societies will accumulate more special-interest organizations as time goes by." As we have seen, this characteristic of interest groups has received the most empirical attention even if it has been poorly measured.

Importantly, Olson's analysis addresses at least two other factors that have received little attention in empirical evaluations of the model. The *second* factor is what has been called the *encompassingness* of the interest groups. Encompassing groups—those that embrace a large proportion of the members of the society—are argued to be more willing than less encompassing groups to pursue policies associated with enhancing competition so as to promote economic efficiency. For such groups, the pursuit of narrow protectionist policies would hurt more members than it might benefit (Olson 1983, 22–23).

Third, Olson suggests that beyond number and encompassingness, the strength of interests vis-à-vis the government is a major characteristic defining the interest group's ability to effect change for its benefit. "The argument here accordingly focuses," Olson (1983, 23) writes, "not only on the absolute power of special-interest groups but also on the *relative* strength of the most encompassing group, the government, versus constituent groups that are much less encompassing." Thus, the characteristics of interest groups alone are not enough to describe interest-group power fully; that power must be defined in relative terms, taking account of governmental power.

This aspect of Olson's model has been most fully developed by Whiteley (1983; see also Lange and Garrett 1985), who has suggested that "a hidden implication of Olson's model which he fails to bring out, is that the government is the only agency in society with an interest in eliminating retardants to growth." Moreover, Whiteley argues that governmental power should be interpreted in terms of control by parties of the Left. He notes (1983) that "the left is much more likely than the right to intervene in the management of the economy to offset the tendency towards stagnation in advanced capitalism." Whiteley finds some support for this hypothesis in tests using cross-national data. However, Whiteley examines only patterns of party control, not the *relative* power of interests so central to Olson's interpretation.

While these three characteristics are those cited by Olson as essential elements of interest-group power, some have argued that at least one other element must be considered in fully specifying the model. Specifically, Dean (1983) has suggested that the *relative* power of different interest groups must be considered. Noting that Olson and his students largely ignore the role of business interest groups, Dean points to at least two kinds of conflict between groups that can serve either to restrict or to enhance the hypothesized interest-group effect on growth: labor-business conflicts and progrowth business–antigrowth business conflicts. As Dean (1983, 254) notes, "Independently of fragmentation, interest groups should be classified according to the nature of their goals and activities . . . and their relative political strengths. The balance of power in this political dynamic varies crucially between countries and over time; in fact it is, I would venture, the critical element in the growth process." Thus, it is not enough to know the number of groups.

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We need to be attentive to interest-group goals on economic growth and their relative power.

To this point, nothing has been said about the *size* of interest groups, the measure of interest-group influence that has actually been used in existing tests of the Olson construct. As we have seen, group size is an inappropriate indicator of the concept it was purportedly measuring —the number of groups. There is simply no necessary relationship between group number and group size. However, this does not mean that group size is an unimportant dimension of interest-group influence. Although Olson's model says little or nothing about group size, it would seem plausible to expect that larger groups would be more politically influential than smaller groups, all other things being equal. Thus, group size is posited as a *fifth* dimension of interest-group influence.

We have, then, five characteristics of interest groups, each of which must be linked to the larger model presented in Equations 2–5. Starting with Equation 2, how should each of these characteristics be related to passage of time since invasion or domestic upheaval? Unfortunately, the answer is clear for only the first of the interest-group characteristics—the number of groups. As we have seen, Olson provides a compelling account of how the number of groups accumulates over time as successive collections of interests overcome the costs of organization. Thus, we specify Equation 6:

$$\text{NUMBER}_i = a_{21} + b_{21}\text{TIME}_i + e_i, \qquad (6)$$

where NUMBER is the number of groups and b_{21} is expected to be positive.

The second characteristic is the encompassingness of groups. But it is not at all clear how the passage of time should influence this characteristic of interest-group organization. Olson's account (1982, 91) of encompassingness addresses only cross-sectional determinants of this trait, arguing that "small and relatively homogeneous societies obviously would be more likely to have organizations that are relatively encompassing in relation to the society than would large and diverse societies." Indeed, Olson (1983, 91) explicitly cautions against expecting any relationship between time and encompassingness, noting that "leaders of special interest organizations . . . cannot get any of the gains that might result from the mergers that could create a more encompassing organization by 'selling' their organization; a merger is indeed likely to result in the elimination or demotion of some of the relevant leaders. There is, accordingly, no inexorable tendency for encompassing organizations to replace narrow ones." Brace (1985), on the other hand, has argued that encompassingness will increase over time; in particular, parties and legislatures will take on the role of encompassing groups. Thus, any expectations about the relationship between this aspect of interest-group influence and time must remain open. Still, we can test the following equation as an analogue of Equation 6:

$$\text{NONENCOM}_i = a_{22} + b_{22}\text{TIME}_i + e_i, \qquad (7)$$

where NONENCOM is the degree to which interest groups are nonencompassing. (Encompassingness is inversely measured so as to facilitate its inclusion in a more complex measure of interest-group power.) However, we have no precise expectation of the sign of the coefficient b_{22}.

Similarly, we have no initial expectations about the third and fourth characteristics of interest groups—the power of interest groups *relative* to the government and to each other. Olson's discussion of the former again stresses cross-sectional variation in the power of interest groups relative to the government, not temporal factors. And as Dean has suggested,

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Olson says little or nothing about the relative power of different groups. Still, it would seem that some sort of temporal development would be consistent with Olson's larger vision of the mechanics of group organization. That is, as groups interested in growth-retarding policies are successful, the potential benefits to be derived from organization by those harmed by these policies would increase. This increase in the potential rewards of organization should help in overcoming some of the costs of organization. Thus, we might expect to see the growth of interest power to lead to the growth of countervailing power, whether through government or private interests. Thus, similar to Equation 7, we have

$$\text{RELGOV}_i = a_{23} + b_{23}\text{TIME}_i + e_i \qquad (8)$$

$$\text{RELINT}_i = a_{24} + b_{24}\text{TIME}_i + e_i, \qquad (9)$$

where RELGOV is the power of interest groups relative to government and RELINT is the power of interest groups favoring protection from the rigors of competition relative to other interest groups less supportive of those actions. While not explicit in Olson's presentation of this model, we would expect the coefficients b_{23} and b_{24} to be negative in sign over the recent short term. While interest-group power and influence would have to increase before the incentives of countervailing groups would be sufficient to overcome the costs of organization, the realization of interest-group power of the type suggested by Olson would increase those incentives.

The analysis of the impact of time since invasion or domestic upheaval on group size is similar to that of the two relative power terms in the model. That is, Olson's model addresses how groups overcome the costs of initial organization, not how they grow over time. Still, the logic of his analysis would seem to suggest that groups have some incentive to grow as individual interest groups if not through merger into more encompassing groups. In contrast to the possible lack of incentives of interest-group leaders to merge their groups with others to form more encompassing groups, the benefits accrued from growth in the size of a single group could be realized by the leaders of the group and would seem to provide a powerful motive for interest-group expansion. This relationship can be expressed in the equation

$$\text{SIZE}_i = a_{25} + b_{25}\text{TIME}_i + e_i, \qquad (10)$$

where SIZE is the average size of interest-group memberships. While not explicit in Olson's presentation, we would expect b_{25} to be positive.

If Equations 6 through 10 provide for a more complete account of the impact of the passage of time on characteristics of interest-group influence, what about the impact of those influence characteristics on the adoption of policies designed to shelter groups from the crosscurrents of unregulated competition—Equation 3? We would expect all five influence traits to be associated with such policy adoptions, but not in a simple additive manner. That is, we expect that several of the dimensions of interest-group influence would combine to generate some level of political pressure for the adoption of growth-retarding policies in the political process.

Starting with the groups supportive of such policies, we would expect their absolute influence—POWER—to be reflected in the interaction of group SIZE and NUMBER. Both the number and size of groups are traits that would be expected to translate into political influence. There are, of course, other resources of groups that would also have a direct bearing on their level of political influence, including, especially, campaign contributions. Maitland (1985) has provided an interesting analysis of Political Action Committee (PAC) contributions in the context of

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Olson's model at the national level. We were not able, however, to develop measures of such resources at the state level. Therefore, our measure is limited to the influence effects of the size and number of groups. Schlozman and Tierney (1986) have argued that the number of separate groups is particularly important in measuring the influence of business interests, a degree of influence that is missed when membership size alone is considered. We start our specification of Equation 11, then, by initially defining *interest-group influence* as the term $[(\text{ANUMBER}_i)(\text{ASIZE}_i)]$, where ANUMBER is the number of antigrowth interest groups and ASIZE is the size of the antigrowth interest groups.

However, the degree to which these groups are free to pursue antigrowth policies is also influenced by the level of group encompassingness. That is, encompassing groups, because they are less homogeneous in membership, must balance the needs of all of their members and are, therefore, less free to pursue more narrow antigrowth policies. This constraint on the ability or willingness (or both) of the antigrowth groups to pursue antigrowth policies can be included as an additional interaction term so that the final measure of the level of political pressure for antigrowth policies (POWER) can be expressed by the term $[(\text{NONENCOM}_i)(\text{ANUMBER}_i)(\text{ASIZE}_i)]$, where NONENCOM is the degree of nonencompassingness of the antigrowth groups. High nonencompassingness would serve to enhance the level of pressure for antigrowth policies beyond that determined by group size and group number alone.

The fourth dimension of influence to be addressed is the relative balance of power between groups supportive of and groups opposed to antigrowth policies—RELINT in Equation 9. The absolute influence of progrowth groups, like that of the antigrowth groups, would be reflected in an interaction of group size and number. Therefore, we can more precisely measure RELINT—the relative influence of progrowth and antigrowth interest groups—with the ratio term $[(\text{NONENCOM}_i)(\text{ANUMBER}_i)(\text{ASIZE}_i)]/[(\text{PNUMBER}_i)(\text{PSIZE}_i)]$, where PNUMBER and PSIZE are the number and size, respectively, of the progrowth interest groups.

The final factor is the power of the antigrowth groups relative to the government, RELGOV in Equation 8, which is included in the model in a manner similar to the relative interest-group-power term. That is, following Whiteley and measuring governmental strength in terms of control by the party of the Left, the relative power of antigrowth interest groups vis-à-vis the government as the largest encompassing group can be measured by the ratio variable $[(\text{NONENCOM}_i)(\text{ANUMBER}_i)(\text{ASIZE}_i)]/[\text{LEFT}_i]$, where LEFT is the level of control of government by leftist parties.

Thus, we can reformulate the model of antigrowth policy adoption—$\text{POLICIES}_i = a_3 + b_3\text{INTERESTS}_i + e_i$—in the following manner:

$$\begin{aligned}\text{POLICIES}_i = {} & a_3 + b_{31}[(\text{NONENCOM}_i)(\text{ANUMBER}_i)(\text{ASIZE}_i)] \\ & + b_{32}[(\text{NONENCOM}_i)(\text{ANUMBER}_i)(\text{ASIZE}_i)]/[(\text{PNUMBER}_i)(\text{PSIZE}_i)] \\ & + b_{33}[(\text{NONENCOM}_i)(\text{ANUMBER}_i)(\text{ASIZE}_i)]/[\text{LEFT}_i] + e_i, \qquad (11)\end{aligned}$$

where the three independent variables measure, respectively, the absolute level of pressure for antigrowth policies (POWER), the degree of that pressure relative to the countervailing pressures of progrowth interests (RELINT), and that relative to the countervailing pressures of government (RELGOV). We would expect b_{31}, b_{32}, and b_{33} to be positive.

Importantly, though, the likelihood of adopting antigrowth policies does not have a simple relationship to the passage of time (since statehood, domestic up-

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heaval, or invasion), as hypothesized by Olson. As we have seen, time since destruction of accumulated interest groups supportive of antigrowth policies is differentially related to the several variables in Equation 11, with time hypothesized to be positively related to ASIZE and ANUMBER, negatively related to RELGOV, PGROWTH, and PSIZE, and ambiguously related to NONENCOM. Surprisingly, then, the indirect effect of time on the adoption of policies could be the opposite of that suggested by Olson if its main effects are realized through the relative power terms in the equation or if time leads to the development of more encompassing groups.

In sum, this more complete specification of the way in which interest groups are expected to matter both suggests that existing empirical tests are very incomplete and raises serious questions about the kinds of expectations the theory should generate about policy adoptions.

The Problem of Testing the Model Empirically

While the two previous problems concern the theoretical specification of the Olson construct, the remaining problem set concerns the appropriate measurement of the variables in the model and the testing strategies used to evaluate it. At least two major problems of this type can be identified.

First, the previous tests of the Olson construct employ an excessively long period of time, and thus weaken any causal link that one could draw between time and policy adoptions inhibiting growth. As Pryor (1983, 91) has noted, "Linking relative growth patterns between 1965 and 1974 to events occurring more than a century before, when industrialization was not far advanced in the states, is a dangerous procedure unless one can obtain evidence that intervening events did not greatly influence current growth patterns." Therefore, it is essential that tests of at least some portions of the model—especially Equations 4, 5, and 11—be restricted to more limited periods of time and focus on specific adoption patterns of growth-related policies in order that a more rigorous interpretation of temporal cause and effect relationships might be made. Moreover, at least one study (Brace and Dudley 1985) has shown that the Olson hypothesis holds up only for states admitted prior to the Civil War. For post–Civil War states, the relationship between time and growth disappears.

Second, the Olson construct has been tested to date largely with simple cross-sectional research designs of the type criticized by Gray (1976). Yet, the model is a theory about change. Change in interest-group power is expected to lead to changes in policy. These changes in policy are then expected to be associated with changes in economic behaviors. Finally, such changes in economic behaviors are expected to be related to changes in economic growth rates. This dynamic is lost in tests employing static measures of interest-group membership. Thus, in developing linkages between the several variables cited in the construct, the use of change measures would seem to be essential.

Testing the Olson Construct

Dynamic Models of Olson's Hypothesis

The last two considerations suggest that Equations 4, 5, and 11 be reformulated as change-score equations with the dependent variables measuring, respectively, changes in policies, economic behaviors, and growth rates over relatively short periods of time to allow for more precise determination of the cause-and-effect relationships posited in the Olson model. The absolute level of the dependent variables at the beginning of the period

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should also be included as independent variables to control for the effects of other long-term forces influencing growth, such as those cited by Pryor (1983, 91). The independent variables of interest should then be included as change scores to capture the short-term forces governing the several processes in question.

However, while we believe that such a revision of the four-equation specification of the Olson construct presented earlier is the most theoretically valid rendering of the model, we test a somewhat simpler specification for two reasons. First, the distinguishing characteristic of the Olson explanation of growth rates is its emphasis on the role of interest groups. Therefore, we focus on that characteristic. Second, while support for all four equations is necessary to validate the Olson model, the requirements of falsification are much less rigorous. That is, if we do not find that the interest group variables are related to economic growth rates in the hypothesized manner, one of the linkages between interest groups and growth rates will be seriously weakened. Given the results, this simpler strategy is appropriate. Thus, our respecified model collapses Equations 4, 5, and 11 such that

$$\begin{aligned} \mathrm{D(GROWTH}_i) = {} & a_3 + b_{31}\mathrm{LAGGROWTH}_i \\ & + b_{32}\mathrm{D[(NONENCOM}_i)(\mathrm{ANUMBER}_i) \\ & (\mathrm{ASIZE}_i)] + b_{33}\mathrm{D[(NONENCOM}_i) \\ & (\mathrm{ANUMBER}_i)(\mathrm{ASIZE}_i)]/[(\mathrm{PNUMBER}_i) \\ & (\mathrm{PSIZE}_i)] + b_{34}\mathrm{D[(NONENCOM}_i) \\ & (\mathrm{ANUMBER}_i)(\mathrm{ASIZE}_i)]/[\mathrm{GOVPOWER}_i] \\ & + e_i. \end{aligned} \tag{12}$$

Lagged values of the dependent variables have been introduced into the equation to account for its level at the beginning of a more narrow period of time and all of the substantive independent variables are included as change scores, as indicated by the D prefix.

Data and Time Period

We have argued, as has Pryor (1983, 91), that the Olson construct should be tested using more discrete time periods. Therefore, the growth-rate variables are measured for the period 1977–82, while the interest-group variables are measured for the period 1975–80. While these more discrete periods and lags differ considerably from those used by Olson and Choi, they are identical to those used by Dye (1980) in his assessment of the Olson model, although his data were from the early 1970s. And, importantly, Dye found substantial support for the Olson model using such time periods and lag structures. Also, Lange and Garrett (1985) have found support for the Olson model in cross-national analyses using growth-rate measures covering a six-year period. Thus, if less supportive results are found here, it should not be due to the use of our specific period and lag structures.[1]

Complete data were not available for all states for all variables. Specifically, data on one or more of the variables is missing for Alabama, Alaska, Hawaii, Iowa, Nebraska, Nevada, Rhode Island, Utah, and West Virginia. Therefore, only 41 cases are employed in the tests to follow.

Measures of the Dependent Variable

As reported in the Appendix, the indicators of the ultimate dependent variable —GROWTH—include two of the measures used by Olson (1983) and Choi (1979): LPI, the exponential rate of growth of income of labor and proprietors from 1977 to 1982; and NPI, the exponential rate of growth of private nonfarm income from 1977 to 1982. Additionally, we employ MPI, the exponential rate of growth of manufactures income from 1977 to 1982, as an additional measure of growth, given the important role of the manufacturing sector. Olson and Choi also tested

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per capita measures of these three growth variables. Accordingly, we also test the growth models using PCLPI, PCNPI, and PCMPI.

Measures of the Independent Variables

The measure of time since major domestic upheaval or war used by Olson and Choi—the number of years since statehood in the case of nonsouthern states and the number of years since 1865 in the case of states in the Confederacy—is used here in all of the tests to follow.

The interest-group measures are more complex for two reasons. First, given Dean's (1983, 254) comments reported earlier about which groups might be expected to be supportive of growth-retarding policies, we examine two separate sets of interests. The first is unions, the group addressed most extensively in Olson's analysis. Second, the role of business interest groups are examined. As Whiteley (1983) has suggested, "When discussing these groups [Olson] focuses almost exclusively on trade unions and the farm lobby. However, any commercial organization has an incentive to achieve monopolistic control of its market or suppliers if it can, and collude with other organizations in order to achieve monopoly profits. Private corporations are just as likely to act in restraint of competition, and hence to retard economic growth if they get the chance."

Second, the interest-group measures are complex because of the need to examine both static and change-score indicators. Olson employed only the former, to the extent that he examined interest groups at all. We have argued that change scores should also be employed to assess the model more validly. Therefore, tests using both are employed.

The first of the interest-group variables is group NUMBER. The number of union and business interest groups in 1975 were obtained from Marquis Academic Media (1978). The number in 1980 were collected by Conover and Gray (1983) from lists of organizations that employ registered lobbyists in state legislatures.[2] These indicators thus have the advantage of measuring the number of interest groups where they come into contact with the political system, something that should be useful in evaluating the impact of such groups on the adoption of growth-restrictive public policies. They have the additional advantage of being objective measures rather than reputational measures of influence in state legislatures. Determination of the number of union (UNUMBER) and business (BNUMBER) organizations employing registered lobbyists from such lists was straightforward. Sets of indicators for both groups were developed for 1975, 1980, and the change from 1975 to 1980.[3]

Group SIZE was most easily measured for unions, with USIZE indicated by the percentage of the labor force that is unionized. Comparable measures for business groups were not available. Therefore, the size of business (BSIZE) interest groups were measured by the percentage of the state's labor force employed in manufacturing and services. While this indicator is hardly comprehensive, it should serve to distinguish between states with very large and very small business sectors. Again, separate size measures were developed for 1975, 1980, and the change from 1975 to 1980.

The level of nonencompassingness of unions (UNONENCOM) is measured by the ratio of the number of union organizations employing registered lobbyists to the size of the unionized sector of the labor force. A high value of the ratio would indicate that there are a relatively large number of unions relative to the number of unionized workers. If we assume that the distribution of workers across the unions is not excessively skewed, then this would indicate that

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each union would represent a relatively smaller proportion of the workers or that each must represent a fairly discrete set of constituents. This, of course, would be the case of high nonencompassingness. Conversely, a low value of the ratio would indicate that the workers are represented by a small number of unions. If we assume that the states do not vary too excessively in terms of the kinds of industries they have, then this would imply that the unions are more encompassing. Obviously, both of these assumptions—on the nonskewed populations of different unions and the general distribution of different types of industries across the states—are questionable. Thus, our index probably misrepresents the nonencompassingness of unions in states with a single dominant industry, as Montana and Michigan had for many years. Still, given the growing diversification of state economies, these assumptions are less tenuous for the period examined here than they might have been for previous decades.

A measure of business-interest-group nonencompassingness (BNONENCOM) was developed through analysis of the relative number of business-firm and business-trade-association organizations employing registered lobbyists. That is, trade associations are definitionally more encompassing than individual firms. Therefore, the level of business nonencompassingness was measured by the ratio of the number of firms employing registered lobbyists to the number of trade associations employing lobbyists. Indicators of the union and business nonencompassing variables were developed for 1975, 1980, and the change from 1975 to 1980.

The multiplicative interaction POWER indicators for the two groups—UPOWER and BPOWER—are also presented in the Appendix. Again, separate indicators were developed for 1975, 1980, and the change from 1975 to 1980.

The relative-interest-group POWER indicators for 1975, 1980, and the change from 1975 to 1980—RELINT—are simply the ratio of each group's POWER indicator to that of the group most likely to conflict with it. In the case of businesses and unions, each serves as the most likely reference for the other. Thus, the indicator of union power relative to that of business is URELB, the inverse of BRELU, the indicator of business power vis-à-vis unions.

Finally, several choices were available in developing an indicator of interest-group power relative to that of government: RELGOV. The appropriateness of the characteristic Whiteley viewed as most relevant for inhibiting growth-restrictive policies—control of government by liberal parties—is of doubtful utility in analyses of the U.S. states given the relatively nonideological character of state politics. Still, we initially followed Whiteley's lead by using as RELGOV indicators the ratio of the 1975 power terms—UPOWER75 and BPOWER75—to an index of liberal control of state government from 1976 through 1980.[4] The index ranges in value from 1 for complete liberal-party control to 0 from complete nonliberal-party control.

We also tested two other indicators of liberal-party control as the denominator of the RELGOV measures: Klingman and Lammers's (1984) index of general policy liberalism and Wright, Erikson, and McIver's (1985, n.d.) index of state public-opinion liberalism. These last two indexes were moderately correlated (.65), though neither was strongly correlated with the liberal-party control index: .08 and .06 respectively. Still, all three indicators generated very similar results when included in the models to be discussed. In the end, the Klingman and Lammers index was used because it was constructed with measures of actual policy intervention on the part of the states. As Klingman and Lammers (1984) note, their analysis of the index "provides fairly strong evidence in

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Table 1. OLS Estimates of Relation between Time since Statehood or Civil War and Six Measures of Economic Growth, 1977–82

Exponential Rate of Growth, 1977–82	Independent Variable (TIME) *b*	Standard Error	Constant	R^2
Labor and proprietors' income	-.032*	.012	2.27	.15
Per capita labor and proprietors' income	.007	.010	1.48	.01
Private nonfarm income	-.033**	.012	2.28	.16
Per capita nonfarm income	.008	.010	1.48	.02
Manufacturing income	-.034*	.015	2.20	.12
Per capita manufacturing income	.007	.011	1.39	.01

Note: Number of cases = 41.

*$p < .05$.
**$p < .01$.

support of the proposition that tendencies toward more or less eagerness in the use of state government may pervade a variety of policy areas."

They also argue, however, that such tendencies are "stable" and "enduring," characteristics that raise a potential problem in the present analysis. Specifically, if we accept their assertion that proclivities for state intervention are stable and enduring, the denominators in our indicators of interest-group power relative to state governments are temporal constants. Thus, the Klingman and Lammers' index is used as the denominator in both the 1975 and 1980 RELGOV measures.[5]

The various models—Olson's original model, Equations 6–10 relating time and interest-group characteristics, and Equation 12, relating economic growth to the interest-group characteristics, were estimated using ordinary least square (OLS) regression analysis. Potential collinearity problems in the estimates of Equation 12 were examined by regressing each of the independent variables on the remaining independent variables. The *R*-square values from these regressions were generally modest, falling in all but a few cases below .50.[6]

Findings

Time and Economic Growth. The results replicating those of Olson and Choi for the 1977–82 period are presented in Table 1. As expected, the number of years since statehood or the Civil War is negatively related to all three of the *total* growth-rate measures. Beyond this similarity, however, the results diverge significantly from those presented by Olson and Choi for the 1965–76 period or by Dye for the 1972–76 period. First, the TIME coefficient is positive though not significant at the usual criterion levels in all of the regressions using per capita measures of growth as the dependent variable. Second, the *R*-square values reported for the *total* growth-measure models are much smaller than those reported by Olson, Choi, and Dye. The *R*-square values of the equations using the *total* growth-rate dependent variables fall far short of the .30–50 range reported for the 1967–76 period or the .42–.54 range for the 1972–76 period, with none exceeding .17. And it is not at all clear which measures—the total or the per capita—are most appropriate. Olson and Choi make no special claim for either and used both. In any case, at least part of their analysis is not replicated, suggesting

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that their results might be an artifact of the particular years they studied.

Time and Interest-Group Characteristics. Turning to the tests of the more complete specification of the Olson model, the results from regressing on TIME the various indicators of characteristics of interest-group influence, Equations 6–10 are reported in Table 2. We had hypothesized that TIME would be positively related to the number of interest groups. As seen in the results presented at the top of the table, only very weak support was provided for this hypothesis. While the coefficient is positive in some cases, the sign of the TIME coefficient in the tests using the 1975 measures of NUMBER are often different from those reported for tests employing the 1980 or change-score measures (or both) of NUMBER. Moreover, the reported *R*-square values are very modest, suggesting that whatever relationship there is is very weak.

Slightly more supportive results were found for the SIZE variable. As seen in the next set of results presented in Table 2, the size of the unionized portion of the labor force in both 1975 and 1980 was positively related to time since the Civil War or statehood as expected, and both coefficients were discernibly different from zero. However, the change in the size of the unionized labor force from 1975 to 1980 was negatively related to TIME, though this coefficient was not significant at the .10 level. For the size of the business-interest-sector measures, the TIME coefficient was negative for 1975 but not significant. Both of the other business-model TIME coefficients were positive as expected, and the 1980 TIME coefficient was significant. Again, though somewhat higher than those reported for NUMBER, the *R*-square values reported for the SIZE equations are quite modest.

No strong expectations were developed about the relationship between TIME and the level of encompassingness of interest groups as measured by NONENCOM, though we have discussed the competing hypotheses of Brace (1985) and Olson (1982, 91) on this issue. And as seen in Table 2, a negative coefficient was generated for five of the six NONENCOM equations, indicating that union and business interest groups are more encompassing in states that have enjoyed the freedom of democratic organization the longest. And while the TIME coefficient is significant in only one case and the *R*-square values are again modest, these findings tend to support Brace's (1985) position on this issue rather than Olson's (1982). And they also suggest that the passage of time since war or domestic upheaval does not have a unidimensional impact on interest-group power. If interest groups do become more encompassing over time, this should weaken their ability or willingness to pursue growth-restrictive policies, even as their size and number increase.

Turning to the interactive-term measures of group power, there seems to be very little relation between the number of years since the Civil War or statehood and interest-group power. Contrary to expectations, all three TIME coefficients in the tests employing dependent measures of union power and one of the coefficients in the business-group power tests are negative, though none are significant. Moreover, none of the *R*-square values in the POWER equations exceed .12, with most falling short of even that modest mark.

The power of interest groups supportive of growth-restrictive policies relative to those opposed to such policies was hypothesized to be negatively related to the number of years since the Civil War or statehood; the organization of the former was presumed to increase the rewards from organization of the latter and thus to encourage their formation. Part of the difficulty of assessing this effect lies in specifying which groups belong in each category, a difficulty that is especially appar-

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Table 2. OLS Estimates of Relation between Time since Statehood or Civil War and Interest-Group Characteristics, 1975, 1980, and 1975–80

Dependent Variable		Independent Variable (TIME) *b*	Standard Error	Constant	R^2
Number of interest groups	UNUMBER75	-.024	.053	27.65	.01
	UNUMBER80	-.016	.066	26.01	.00
	UNUMBERCH	.000	.064	-1.64	.00
	BNUMBER75	-.131	.283	136.57	.01
	BNUMBER80	.384	.412	149.78	.02
	BNUMBERCH	.515	.327	13.21	.06
Size of interest groups	USIZE75	.001***	.000	.09	.21
	USIZE80	.001***	.000	.10	.20
	USIZECH	-.000	.000	.17	.02
	BSIZE75	-.026	.018	4.52	.05
	BSIZE80	.001***	.000	.25	.24
	BSIZECH	.027	.017	-4.27	.06
Nonencompassingness of interest groups	UNONENCOM75	-.758**	.300	227.93	.14
	UNONENCOM80	-.614	.547	207.12	.03
	UNONENCOMCH	.144	.464	-20.81	.00
	BNONENCOM75	-.001	.001	.93	.01
	BNONENCOM80	-.003	.002	1.67	.06
	BNONENCOMCH	-.002	.001	.74	.05
Absolute interest group power	UPOWER75	-1.04	2.99	913.49	.00
	UPOWER80	-3.11	6.41	1252.72	.01
	UPOWERCH	-2.07	5.86	339.24	.00
	BPOWER75	-.393	.266	103.32	.05
	BPOWER80	.494	.367	41.13	.04
	BPOWERCH	.887**	.406	-61.19	.11
Interest-group power relative to other interest groups	URELB75	.002	.002	.41	.02
	URELB80	-.001	.001	.18	.02
	URELBCH	-.002	.002	.22	.03
	BRELU75	-.002	.002	.41	.02
	BRELU80	.001	.001	.18	.02
	BRELUCH	.002	.002	.22	.03
Interest-group power relative to government	URELGOV75	19.23**	8.90	-2749.40	.11
	URELGOV80	18.49	16.18	-2798.41	.03
	URELGOVCH	-.74	13.05	-49.01	.00
	BRELGOV75	2.22***	.60	-307.05	.26
	BRELGOV80	4.07***	1.31	-507.79	.20
	BRELGOVCH	1.86*	1.01	-200.74	.08

Note: Number of cases = 41.

*$p < .10$.
**$p < .05$.
***$p < .01$.

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Table 3. OLS Estimates of Relation between 1975 Measures of Labor Interest-Group Strength and Economic Growth, 1977–82

	Independent Variable					
Exponential Rate of Growth	LAG77[a]	1975 Absolute Union Power UPOWER75	1975 Unions Relative to Business URELB75	1975 Unions Relative to Government URELGOV75	Constant	R^2
Labor and proprietors' income	-.00001 (.00000)	.0008 (.0007)	-3.31*** (.95)	-.0006*** (.0002)	1.73	.41
Per capita labor and proprietors' income	.083 (.671)	.0002 (.0006)	-1.84** (.92)	-.0001 (.0002)	1.49	.11
Private nonfarm income	-.00001 (.00000)	.0008 (.0007)	-3.07*** (.94)	-.0006*** (.0002)	1.75	.39
Per capita private nonfarm income	.44 (.61)	.0001 (.0006)	-1.72* (.88)	-.0001 (.0002)	1.32	.11
Manufacturing income	-.0002** (.0001)	.0023*** (.0008)	-2.67** (1.22)	-.0001 (.0002)	1.68	.32
Per capita manufacturing income	-.02 (.85)	.0008 (.0006)	-1.78* (.98)	.0001 (.0002)	1.40	.11

Note: Number of cases = 41. Figures in parentheses are standard errors.

[a]LAG77 is the 1977 value of the dependent variable.

*$p < .10$.
**$p < .05$.
***$p < .01$.

ent in the use of inverse measures of the business-union relationship in the business and union RELINT equations presented in Table 2. The expected negative coefficients were generated for two of the coefficients in the union equations and one in the business-interest-group equations. However, none were significant at the .10 level. In general, though, the *R*-square values associated with these equations are so small as to suggest that no strong conclusions about the relation between TIME and RELINT should be drawn from these results.

The final results presented in Table 2 address the relationship between TIME and the power of interest groups relative to the level of liberal-party control of government. Mixed signs were again found, with TIME negatively related to URELGOVCH and positively related to the other dependent measure. And the positively signed coefficients are significant in all but one case. Again, however, the *R*-square values for these equations are not especially strong. Still, the weight of evidence suggests that time is positively associated with group power relative to that of government as Olson implied. Indeed, these findings provide the best support for Olson's view of the impact of time on group influence.

In sum, though, and contrary to the assumptions underlying Olson and Choi's tests, time since statehood or Civil War seems to be neither strongly nor consistently related to the several dimensions of interest-group influence. However, the dimensions of interest-group influence could still be related to economic growth rates in the manner specified in Equation 12.

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Table 4. OLS Estimates of Relation between Change Measures of Labor Interest-Group Strength and Economic Growth, 1977–82

Exponential Rate of Growth	Independent Variable: LAG77[a]	Change of Absolute Union Power UPOWERCH	Change of Unions Relative to Business URELBCH	Change of Unions Relative to Government URELGOVCH	Constant	R^2
Labor and proprietors' income	-.00002 (.00001)	-.00002 (.00050)	2.76*** (.91)	-.0003 (.0002)	1.92	.32
Per capita labor and proprietors' income	.047 (.646)	-.0001 (.0004)	1.58* (.81)	-.0001 (.0002)	1.57	.11
Private nonfarm income	-.00002 (.00000)	-.00003 (.00040)	2.51*** (.88)	-.0003* (.0002)	1.93	.31
Per capita private nonfarm income	.44 (.58)	-.0001 (.0004)	1.44* (.78)	-.0001 (.0002)	1.37	.11
Manufacturing income	-.0002** (.0001)	-.0004 (.0006)	1.71 (1.12)	-.0005** (.0002)	1.89	.27
Per capita manufacturing income	.19 (.80)	-.0003 (.0005)	1.19 (.87)	-.0003 (.0002)	1.48	.10

Note: Number of cases = 41. Figures in parentheses are standard errors.

[a]LAG77 is the 1977 value of the dependent variable.

*$p < .10$.
**$p < .05$.
***$p < .01$.

Economic Growth and Interest-Group Characteristics. These results are presented in Tables 3–6. The union findings reported in Table 3 employ the *static* 1975 measures of UPOWER and URELB while those reported in Table 4 employ the *change* from 1975 to 1980 measures of those variables. Starting with the former, the results presented in Table 3 provide only very weak support for the Olson construct, which would lead us to expect that the UPOWER75, URELB75, and URELGOVCH coefficients would be uniformly negative accross the equations using the several measures of the rate of economic growth. But the UPOWER coefficients are uniformly positive, though only one was significant. While very weak, these results suggest that if anything, higher levels of union power were associated with *higher* rates of economic growth. In contrast, though, the URELB75 coefficients were negative as expected and significantly different from zero in all six equations. All but one of the URELGOVCH coefficients were negative as expected, and two were discernibly different from zero. Obviously, such results are difficult to interpret. The problem lies in reconciling the conflicting UPOWER75 and URELB75 results. And the most obvious interpretation would be that it is not union power per se that inhibits growth, but union power relative to the power of government and especially business interest groups.

But even this interpretation conflicts with the results presented in Table 4 which employ change-score measures of UPOWER and URELB, measures that we have argued are more appropriate to

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Table 5. OLS Estimates of Relation between 1975 Measures of Business Interest-Group Strength and Economic Growth, 1977–82

	Independent Variable					
Exponential Rate of Growth	LAG77[a]	1975 Absolute Business Power BPOWER75	1975 Business Relative to Labor BRELB75	1975 Business Relative to Government BRELGOV75	Constant	R^2
Labor and proprietors' income	-.00002 (.00001)	.015 (.013)	.001 (1.873)	-.007** (.003)	1.80	.37
Per capita labor and proprietors' income	.13 (.65)	.002 (.011)	1.24 (1.57)	-.002 (.003)	1.47	.11
Private nonfarm income	-.00002 (.00001)	.016 (.013)	-.31 (1.86)	-.007** (.003)	1.81	.34
Per capita private nonfarm income	.47 (.58)	.002 (.010)	1.21 (1.52)	-.002 (.003)	1.31	.11
Manufacturing income	-.0002*** (.0001)	.046*** (.015)	-3.72* (2.16)	.001 (.001)	1.75	.34
Per capita manufacturing income	-.11 (.90)	.017 (.011)	-.25 (1.66)	.003 (.003)	1.43	.13

Note: Number of cases = 41. Figures in parentheses are standard errors.

[a]LAGG77 is the 1977 value of the dependent variable.

*$p < .10$.
**$p < .05$.
***$p < .01$.

assess the cause-and-effect relationship implied in the Olson construct. When the UPOWERCH measure is substituted for UPOWER75, all six coefficients are negative as expected, but none are significant. And all of the URELBCH coefficients change sign from those observed for URELB75, and four were significant at the .10 level. Still, all of the URELGOVCH coefficients carried a negative sign, and again two were statistically significant. At most, these results suggest that the power of unions relative to business is positively related to economic growth rates, the opposite of the conclusion drawn from Table 3.

It remains unclear how union interest-group power is related to economic growth. The union results presented in Tables 3 and 4 provide little support for Olson's claim that union power per se is negatively associated with growth rates. Clearly, union power *relative* to that of business does matter, but how it matters depends on whether one is viewing the static measures in Table 3 or the change measures in Table 4. More unambiguously, it appears that union power *relative* to government is negatively associated with economic growth.

Turning to the business interest-group test results presented in Tables 5 and 6, we find similar results. Table 5 presents the results for the equations using the 1975 measures of BPOWER, BRELU, and BRELGOV. Like the union results, these equations produced uniformly positive BPOWER75 coefficients, indicating that business interest-group power is positively associated with economic growth. All but one of the coefficients were nonsignificant, however. More mixed results were found for the 1975 measure of

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Table 6. OLS Estimates of Relation between Change Measures of Business Interest-Group Strength and Economic Growth, 1977–82

Exponential Rate of Growth	Independent Variable: LAG77[a]	Change of Absolute Business Power BPOWERCH	Change of Business Relative to Labor BRELBCH	Change of Business Relative to Government BRELGOVCH	Constant	R^2
Labor and proprietors' income	-.00002 (.00001)	.025** (.010)	-4.27*** (1.28)	-1.09** (.45)	1.93	.35
Per capita labor and proprietors' income	.09 (.72)	.0014 (.0058)	-1.91* (1.12)	-.0016 (.0017)	1.55	.12
Private nonfarm income	-.00002 (.00000)	.0003 (.0006)	-3.61*** (1.14)	-.0058*** (.0016)	1.94	.32
Per capita private nonfarm income	.57 (.66)	.00001 (.00560)	-1.63 (1.08)	-.0020 (.0016)	1.32	.13
Manufacturing income	-.0002* (.0001)	.0052 (.0084)	-2.95* (1.57)	-.0047** (.0023)	1.88	.26
Per capita manufacturing income	.29 (.91)	.0019 (.0060)	-1.59 (1.20)	-.0019 (.0019)	1.47	.07

Note: Number of cases = 41. Figures in parentheses are standard errors.

[a]LAG77 is the 1977 value of the dependent variable.

*$p < .10$.
**$p < .05$.
***$p < .01$.

business-group power relative to unions; three of the BRELU75 coefficients were negative as expected, and one of these was discernibly different from zero. Like the union results, the BRELGOVCH coefficients were again negative in four of the equations, and two were significant at the .05 level. Generally, these results are quite weak and provide only indirect support for the Olson model in the 3 significant and negative coefficients among the 18 substantively important coefficients.

More supportive results were found for the equations employing change scores for the BPOWER and BRELU terms, as seen in Table 6. Contrary to expectations, the BPOWERCH coefficients are positive, and one is significant. But, as expected, all of the BRELUCH and BRELGOVCH coefficients are negatively signed and more than half are significant at at least the .10 level. Indeed, these results are the most supportive of the Olson construct of those reported here.

Thus, there is less problem interpreting the impact of business interest-group power than there was in the case of union power. It would seem that it is the *relative* power of business interest groups that is negatively associated with retarding economic growth, not business interest-group power per se. If anything, absolute business interest-group power seems to be positively associated with economic growth.

Interpreting the Findings

Quite clearly, these results provide little support for the Olson construct. The dis-

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confirming evidence is both extensive and strong. *First*, we were largely unable to replicate the strong results reported by Olson, Dye, and Choi on the relationship between time since statehood or the Civil War and total growth rates. As seen in Table 1, while the TIME coefficients were negative as expected in the *total* growth-measure equations, they were very weak. Moreover, the per capita measures of growth generated uniformly positive coefficients in Table 1. Thus, our results are more consistent with those of Brace and Dudley (1985) who argue that previous evidence of a relationship between time and growth is spurious. Such a relationship might have appeared because time is highly related with region, and regions have experienced different patterns of growth in part due to federal spending patterns. As federal spending patterns were geographically equalized after the late 1970s (Lowery, Brunn, and Webster 1986), the relationship would be expected to disappear.

Second, there appears to be little or no relation between time since statehood or the Civil War and various measures of interest-group influence. The signs of the coefficients reported in Table 2 varied widely depending on whether 1975, 1980, or change-score measures of interest-group influence were used, and even the supportive findings were very weak.

Third, the interest-group influence-growth rate relationships tested in Tables 3–6 generated results that were more often the opposite of what Olson would lead us to expect than they were consistent with his model. Generally, the absolute power of interest groups was found to be positively related to the several measures of economic growth rates.

At best, some indirect support was provided the Olson construct in the findings that the *relative* power of interest groups was often negatively related to growth. This was strongest for the business interest-group equations for which the support was, in comparison to that for unions, relatively consistent. Thus, Whiteley's expectation that business interest groups would be the major actors in growth-restrictive policies is provided some support by these results. Just as importantly, both union and business interest-group power *relative* to governmental power evidenced a consistently negative association with economic growth. Yet, any support these results provide the Olson model remains incomplete, given the weak relationships between the relative power measures and time since statehood, the underlying engine of the Olson model.

Thus, these results provide little support for the Olson construct as applied to understanding economic growth in the U.S. states. What do these results say, then, about the Olson model more generally? We do not want to suggest that these results totally invalidate the Olson construct. The Olson model is theoretically powerful and may have much to say about cross-national growth rates.[7] Still, we have argued that more is required to validate the model than tests of the simple relationship between time since war or since domestic upheaval and growth rates. The Olson construct is made up of a complex set of relationships, each of which must be empirically supported. Thus, even though more elaborate than tests of previous investigators, even ours is not a full test of the model. We would argue that Olson's model remains largely untested.

This conclusion applies especially to the results of Olson, Dye, and Choi. Their findings on economic growth in the U.S. states have provided much of the presumed support for the Olson model to date. However, they fail to test several key linkages of the model. Moreover, their measures of interest-group influence are rudimentary. When better measures are employed and the key linkages are specified, the supportive evidence for

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Olson in the case of the U.S. states largely evaporates.

We should not be surprised by this. As Pryor (1983, 91) has noted, the time periods involved are simply too long to expect the simple relationships posited by the Olson model to hold. Moreover, much more is involved obviously in the determination of economic growth rates in the U.S. states than is accounted for in the Olson model. From 1977 to 1982, the U.S. states endured the deepest recession of the postwar era, continued to face the growing pressures of international competition, were undergoing major shifts in population from the rust belt to the sun belt, and were actively adopting a number of new economic policies under the rubric of industrial policy. Something more than time since statehood is obviously accounting for the booms and busts experienced by Texas and Massachusetts over the last decade. Neither Choi's test nor those presented here account for any of these factors. While interest groups certainly play some role in economic growth, they are not the sole or perhaps even the most important determinant. In short, the Olson model is too spare and misspecified.[8] Thus, the traditional call for more research is fitting, especially in the context of the current lively debate over "business climate" in the states. Better and more complex tests of the Olson construct are needed. And until those tests are developed, the Olson construct remains an interesting—but unproven—account of varying economic growth rates.

Appendix

The following indicators were used to test the Olson construct:

Economic Growth. LPI = exponential rate of growth of income of labor and proprietors, 1977–82; PCLPI = exponential rate of growth of per capita labor and proprietors' income, 1977–82; NPI = exponential rate of growth of private nonfarm income, 1977–82; PCNPI = exponential rate of growth of per capita private nonfarm income, 1977–82; MPI = exponential rate of growth of manufacturing income, 1977–82; PCMPI = exponential rate of growth of per capita manufacturing income, 1977–82.

Time. TIME = number of years since state admitted to the union (or since the Civil War in the case of Confederate states).

Number of Interest Groups. UNUMBER75 = number of union organizations employing registered lobbyists, 1975; UNUMBER80 = number of union organizations employing registered lobbyists, 1980; UNUMBERCH = (UNUMBER80) − (UNUMBER75); BNUMBER75 = number of business organizations employing registered lobbyists, 1975; BNUMBER80 = number of business organizations employing registered lobbyists, 1980; BNUMBERCH = (BNUMBER80) − (BNUMBER75).

Size of Interest Groups. USIZE75 = proportion of employees of nonagricultural establishments that are unionized, 1975; USIZE80 = proportion of employees of nonagricultural establishments that are unionized, 1980; USIZECH = (USIZE80) − (USIZE75); BSIZE75 = percentage of the labor force in manufacturing or services, 1975; BSIZE80 = percentage of the labor force in manufacturing or services, 1980; BSIZECH = (BSIZE80) − (BSIZE75).

Group Nonencompassingness. UNONENCOM75 = (UNUMBER75)/(USIZE75); UNONENCOM80 = (UNUMBER80)/(USIZE80); UNONENCOMCH = (UNENCOMPASS80) − (UNENCOMPASS75); BNONENCOM75 = (number of business firms employing registered lobbyists)/(number of trade associations employing registered lobbyists), 1975; BNONENCOM80 = (number of business firms employing registered lobbyists)/(number of trade associations employing registered lobbyists), 1980; BNON-

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ENCOMCH = (BNONENCOM80) − (BNONENCOM75).

Absolute Group Power. UPOWER75 = (UNUMBER75)(USIZE75)(UNONENCOM75); UPOWER80 = (UNUMBER80)(USIZE80)(UNONENCOM80); UPOWERCH = (UPOWER80) − (UPOWER75); BPOWER75 = (BNUMBER75)(BSIZE75)(BNONENCOM75); BPOWER80 = (BNUMBER80)(BSIZE80)(BNONENCOM80); BPOWERCH = (BPOWER80) − (BPOWER75).

Relative Group Power. URELB75 = [(BPOWER75)/(UPOWER75)](−1); URELB80 = [(BPOWER80)/(UPOWER80)](−1); URELBCH = (URELB80) − (URELB75); BRELU75 = [(BPOWER75)/(UPOWER75)]; BRELU80 = [(BPOWER80)/(UPOWER80)]; BRELUCH = (BRELU80) − (BRELU75).

Group Power Relative to Government. URELGOV75 = (UPOWER75)/(Klingman-Lammers index); URELGOV80 = (UPOWER80)/(Klingman-Lammers index); URESLGOVCH = (URELGOV80) − (URELGOV75); BRELGOV75 = (BPOWER75)/(Klingman-Lammers index); BRELGOV80 = (BPOWER80)/(Klingman-Lammers index); BRELGOVCH = (BRELGOV80) − (BRELGOV75).

Notes

An earlier version of this paper was presented at the 1986 meeting of the Midwest Political Science Association, Chicago.

1. Specifically, like our measures, Dye's dependent measures of economic growth rates covered a four-year period, 1972–76. And as in our test, Dye's independent variables were lagged two years prior to the beginning of the period covered by the dependent measures—1970. Lange and Garrett's growth measures covered the period 1974–80.

2. Use of lists of registered lobbyists raises the question of the comparability of state laws. Lobbyists are now required to register in 47 of the 50 states. In 35 of the 44 states making up the 1980 data set used here, a lobbyist is defined as anyone receiving compensation to influence legislative action. In the 9 states not using this definition, a more restrictive one is used; a lobbyist is defined as anyone representing someone else's interests or attempting to influence legislation, or both. The difference in definition appears to be a major problem in only one state, Florida. States also make exceptions to their laws, with some exempting members of the media, religious organizations, or persons who testify before committees. These exemptions do not appear problematical. Most states also exempt state officials. For a description of the specific coding procedures used to construct the interest-group measures, see Conover and Gray 1983.

3. We thank Anne Wall for her coding of these data.

4. The index measures the extent of liberal-party control of government via a weighted average of the proportion of years the Democratic party controlled the governorship, House, and Senate in nonsouthern states. The control of the governorship is weighted twice as heavily as that of the House and Senate to reflect the importance of the governor in economic development policies.

5. While the assertion of temporal stability remains questionable, the use of the Klingman and Lammers index is still warranted given its greater face validity relative to the liberal-party control measure and because the Wright, Erikson, and McIver index also rests on an assertion of temporal stability.

6. In the few cases where higher values were observed, approaching .79 in some cases, all involved the POWER variables, something that should not be surprising, given the fact that they are the numerators in the relative power measures. To further evaluate the collinearity threat, Equation 12 was reestimated with only the POWER terms. The signs and significance levels generated by these regressions were generally the same as those reported in the tables. Moreover, the key problem with the findings is the abundance of wrongly signed coefficients and not nonsignificant, though correctly signed, coefficients. This suggests that collinearity is not the source of the failure to find supportive results for the Olson model.

7. Still, even cross-national tests of Olson's model generate mixed results. For example, see the results in the recent special issue of *Scandinavian Political Studies*, where Lane and Ersson (1986) provide confirmatory support for the model and Gustafsson (1986) provides disconfirming evidence.

8. Also, both our work and that of Olson neglects the potentially positive benefits of group activity. Brace and Dudley (1985) argue forcefully that redistributive goals can be more easily achieved in older states which tend to have higher average incomes though lower rates of growth, an achievement Olson ignores. Moreover, some of the more recent and innovative developments in state industrial policy suggest that business and labor can, at least under certain circumstance, cooperate with government to enhance economic growth opportunities.

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Virginia Gray is Professor of Political Science, University of Minnesota, Minneapolis, MN 55455.

David Lowery is Associate Professor of Political Science, University of North Carolina, Chapel Hill, NC 27514.

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INSTITUTIONS, THE ECONOMY, AND THE DYNAMICS OF STATE ELECTIONS

JOHN E. CHUBB
The Brookings Institution

As the U.S. states develop their political institutions and take greater responsibility for their economic well-being, two concerns that have long driven research on national elections—electoral insulation and economic accountability—should become central in research on state elections. I investigate institutionalization's effects on the vulnerability of state elections to major periodic forces—coattails, turnout, and economic conditions—and how political responsibility for economic growth is apportioned between presidents and governors in state elections. The investigation relies upon dynamic models of state legislative and gubernatorial outcomes estimated with a pooled data set comprised of most states and elections in the years 1940–82. The results, which have important implications for state government more broadly, indicate that institutionalization has substantially insulated legislative elections against major threats and that state legislators and governors have less to fear from their state economies than is often thought, but also that state elections are becoming more susceptible to swings in the national economy.

State government, long a backwater of U.S. politics, has been moving steadily into the mainstream. Its institutions have been growing in size, competence, and legitimacy while control of them has become the object of increasing competition. The responsibilities of state government have also increased, and that trend, now driven by fiscal constraints upon the national and local governments, is likely to continue apace. Research, of course, has monitored this progress and described aspects of it in valuable detail. Still, it is fair to say that the implications of state development have not been adequately appreciated, for research on the states has not joined the mainstream itself (Jewell 1982).

The problem arguably begins with our understanding of how state government is constituted, that is, with our understanding of state elections. Much of the progress that state government has been making is a result of a process of political development that has potentially profound implications for state elections. State government has been institutionalizing, and in the process it has been developing not only larger and more complex governing organizations but, perforce, greater amounts of protection from its political environment. Elections are an integral part of this process, for their insulation from powerful political shocks and surges is a virtual prerequisite for institutional stability and development. At the national level, research has amply demonstrated this. Congress did not become thoroughly institutionalized until it was able to stabilize its membership (Brady 1985; Polsby 1968), and its development in recent years has been bound up with the efforts of its members to maintain, if not increase, their electoral secur-

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ity (cf. Fenno 1978; Fiorina 1977; Jacobson 1987). At the state level, analogous efforts have been under way for some time. Yet their electoral consequences, and those of institutionalization more generally, have not been investigated with comparable sophistication or thoroughness. In fact, much the same can be said of research into the forces that regularly influence state-election outcomes and that state politicians are presumably trying to resist.

The strongest case in point is the state economy. Election outcomes have been shown to be affected in a wide variety of contexts by economic conditions (e.g., Alt and Chrystal 1983), and there is ample reason to expect the same to hold true in the states. State politicians must be concerned about the condition of their economies not only now that state responsibilities for social welfare and economic development have increased but because economic conditions affect the ability of states to attract and retain businesses and taxpayers (e.g., Oates 1972; Tiebout 1956). Nevertheless, there is no systematic evidence, at least of the kind that has been amassed in research on national elections, that state economic conditions have electoral consequences (but cf. Peltzman 1987). Models of state elections (e.g., Caldeira and Patterson 1982b; J. Campbell 1986; Patterson 1982; Piereson 1977; Tompkins 1984) have so far attended to factors other than the economy and—returning to the general point—been estimated with seriously limited samples of elections. It remains to be seen, therefore, not only how economic and other influences on state elections have responded to institutionalization but precisely what weights those influences have in the first place.

My purpose is to take a look at these issues with the perspective provided by several fairly comprehensive models of state-election outcomes. Examining legislative elections first, and then gubernatorial, I will introduce macromodels of state-level outcomes. The models will then be used to estimate the influence of basic economic and noneconomic electoral conditions and to assess how their influence varies across time and across states. The models are estimated using a pooled data set that includes most of the states and all of the even-numbered election years in the period 1940–82. The results, which clarify especially the effects of institutionalization and economic performance on state elections, have plain implications for state government more generally. They suggest that the states are systematically overcoming one of their traditional institutional limitations (see, e.g., Key 1949; Wahlke et al. 1962) and that they are less constrained by their own economic circumstances than is often thought (see, e.g., Gramlich and Laren 1984), but that they have become more vulnerable to swings in the national economy.

The Core Model of Legislative Elections

Although state legislative elections exhibit a potentially bewildering degree of diversity, their aggregate dynamics are strikingly regular. In a pattern every bit as consistent as that in congressional elections, state legislative elections have regularly gone against the party of the president at midterm. Only once in the past 50 years has the president's party increased its share of either lower or upper state legislatures in the even-numbered off-year elections. And since 1950 it has always lost, dropping an average net of 13 legislatures in each midterm election (e.g., Bibby 1983). There is some evidence that this pattern is a product of presidential coattails (see, e.g., J. Campbell 1986), but like the pattern that similarly characterizes congressional elections, it may be the result of other national forces as well.

Whatever its source, the similarity be-

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tween national- and state-level election dynamics is potentially important. State elections are extraordinarily diverse. They number over seven thousand and are contested in single- and multimember districts that represent from 23 hundred to 600 thousand voters. The issues that divide the parties vary from state to state and often differ from those that divide the parties at the national level. The factors that account for variations in normal partisan voting across the states include idiosyncrasies of culture and history that subvert general explanations—not to mention statistical models. It is therefore important that at least in their dynamics these diverse elections have something in common with national elections, which, as a group, are well behaved. It raises the possibility that changing party fortunes in state government may be able to be understood—and modeled—in the same familiar terms as changing party fortunes in the national government.

Accordingly, a core model of state legislative elections is specified along the same lines as a well-balanced dynamic model of congressional elections. The Democratic party's percentage of seats in a state legislature is assumed to be a simple linear function of three types of variables that figure prominently in models of national electoral change.[1]

Coattails

Voting in state legislative elections may be influenced by the preferences that voters hold for candidates for higher office: not only president, but governor, and U.S. senator.[2] To capture these influences at the aggregate level, the coattail effects are assumed to be proportional to the strength of state support for the Democratic candidate for each of these higher offices. The Democratic percentages for president, senator, and governor therefore enter the model during each year in which elections for those offices are held. In years when those offices are not up for election, the respective variables take on the value of 0, indicating the absence of coattails. However, to avoid confusing the absence of coattails that arises when a higher office is not at stake with the weakness of coattails that occurs when a higher office is nominally or poorly contested, dummy variables designating presidential, senatorial, and gubernatorial election years are also specified. These variables, taking on the value 1 if the designated office is at stake, and 0 if it is not, permit the estimation of different base levels of Democratic support (by introducing different intercepts) for years when the ballot is occupied by different combinations—none, some, or all—of higher offices. In addition, the model includes a separate measure of the Democratic percentage for governor in nonpresidential years. Its purpose is to allow for the greater influence of gubernatorial voting on state legislative elections when presidential voting is not a factor—a condition that has become more common as gubernatorial races have recently been moved to off years.

Turnout

Changes in levels of voter participation between presidential and nonpresidential election years may have differential effects on the political parties and consequently on the shares of state legislative seats that they win. For a number of reasons (cf. A. Campbell 1967; Kernell 1977) the party of the candidate that wins the presidency tends to benefit from the surge in turnout that typically occurs during the presidential election year and to suffer from the decline in turnout that occurs during the nonpresidential year. *That* state-election turnout varies predictably with political conditions has been established (see, e.g., Caldeira and Patterson 1982a; Patterson and Caldeira 1983), but

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to what effect has not. Here that effect is assumed to occur because marginal voters—those who are stimulated to vote in a presidential-election year but are insufficiently motivated to vote in the subsequent midterm election—vote (and then fail to vote) disproportionately for the lower-office candidates of the president's party. This effect is measured by an interaction between (1) the change in turnout between the current and the prior election and (2) the change in the Democratic percentage for president between the most recent presidential election and the one four years previous.[3] For example, in nonpresidential election years the Democratic party will increase its share of state legislative seats (i.e., the interaction will be positive) if its previous presidential candidate fared worse than its candidate four years before and if turnout declined from the presidential to the midterm election.

National Economic Conditions

The relationship between economic conditions and election outcomes is potentially more complex at the state level than at the national. At the state level the relationship may be a function of the performances of both the state and the national economies and of attributions of responsibility to state officials—for example, governors—as well as to presidents. Still, state election outcomes ebb and flow much like congressional outcomes which depend on the tides of the national economy. It is therefore useful, at least in order to establish a standard for comparison, to estimate the sensitivity of state elections to the national economy—or to the president's perceived performance in managing it. Questions of state-level conditions and responsibilities are best postponed until the simpler one of national effects is answered at least provisionally.

The only question in specifying the more conventional national influence is, What economic heuristic should voters be assumed to be employing? That of course is a matter of some debate in research on congressional elections (cf. Bloom and Price 1975; Fiorina 1981; Kiewiet 1983; Kramer 1971, 1983; Rivers 1986). In research on state elections it is an open question. Here, voter evaluations of presidential performance in managing the national economy are assumed to be based on *changes* in the annual (calendar-year) rate of growth or decline in national real per capita disposable income. These changes are assumed to affect the president's party symmetrically in state elections, economic improvements helping his party as much as deteriorations hurt it. To associate the economic changes with the appropriate party, the income variable is multiplied by an incumbency dummy variable, +1 if the president is a Democrat, −1 if a Republican.

The model specification is completed with a variable (besides the state intercepts discussed below) that establishes a baseline upon which the party builds its share of the legislature in each election. The baseline is indicated by the percentage of seats held by the Democrats after the preceding state legislative election. The coefficient on this variable indicates the rate at which the party retains its share of seats in the state legislature, all things being equal. The effects of coattails, turnout, and economic conditions add to, or subtract from, the level of support that the coefficient of retention establishes by operating on the prior legislative share.

The model was estimated for lower legislatures only; upper legislatures present serious difficulties like overlapping and unequal terms and were therefore excluded. Also omitted were southern states, because until recently their solid one-party systems provided no change to explain; states holding lower-legislative elections in odd years, because they complicate the even-year time series; states

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Table 1. Coefficient Estimates for the Core Model of Control of Lower State Legislatures, Non-South, 1940–82

Variable	Coefficient	*t*-score
Constant[a]	21.02	6.08*
Prior % Democratic seats[b]	.42	7.40*
% Democratic, president	.32	4.90*
Presidential election year	-10.52	3.13*
% Democratic, Senate	.33	6.87*
Senatorial election year	-16.15	6.52*
% Democratic, governor	.35	5.69*
Off-year % Democratic, governor	.08	1.74**
Gubernatorial election year	-20.32	6.80*
Surge or decline	.008	2.98*
Presidential responsibility for change in national income growth[c]	.53	4.70*
Number of cases	751	
R^2	.75	
Adjusted R^2	.74	

Note: The dependent variable is the Democratic percentage of legislative seats by state.

[a]The variable list excludes dummies that were included for 34 states.

[b]The instrumental variables used to estimate this model include all exogenous variables and their two-year lags.

[c]Annual change in the percentage-point growth of national real per capita disposable income.

*$p \leq .05$, two-tail test.
**$p \leq .10$, two-tail test.

with nonpartisan legislative elections; and states with four-year (rather than two-year) terms for the lower house. Still, the data set includes all of the nonsouthern states except Maryland, Minnesota, Nebraska, and New Jersey. The period analyzed extends back from 1982 to 1940; the 1930s were excluded because the economic and electoral volatility of that decade introduced significant statistical outliers that tended to distort estimation. The result is a pooled cross-section time-series data set with 35 states, 22 election years, and 770 cases.[4] The great virtue of such a data set is that it permits the estimation of multivariate models that could not be estimated confidently with the historical time series provided by individual states.

The model was estimated with a limited-information maximum-likelihood program, and the two most likely sources of bias in the parameter estimates were corrected: the coefficient on the lagged endogenous variable, prior percentage of Democratic seats, was estimated with instrumental variables (as indicated in Table 1), and the constant was divided into separate states "fixed effects"—to accommodate different levels of Democratic strength—by specifying a dummy variable for each state (except one that is represented by the constant itself).[5] The fixed-effects specification should also improve the efficiency of the estimates by reducing serial correlation among the errors and heteroskedasticity (Stimson 1985). Further improvements in efficiency were not pursued, but neither were they strongly indicated.[6]

The results, reported in Table 1, are encouraging: they suggest that variations in state-legislative-election outcomes can be well explained by many of the same fac-

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tors that account for variations in national-election outcomes. The model explains most of the variation (74%) in the Democratic share of state legislative seats across states and across time and correctly predicts the party in control of the state legislature in 83% of the cases. Moreover, all of the hypothetical influences on those elections have the correct sign and magnitude, and achieve acceptable levels of significance.

Presidents, senators, and governors all have significant coattails: a 10% increase in the vote for any one increases the Democratic share of seats in the lower house by 3.2%, 3.3%, and 3.5% respectively; off-year gubernatorial coattails add only a little: .8%. By one conventional standard—presidential coattails in congressional elections of roughly 3% (Ferejohn and Calvert 1984; Kramer 1971)—the effects of all higher-office coattails on state legislative elections have been equally sizable.

But by another standard this is not quite so. The coattail coefficients gauge only the marginal influence of votes for higher office on votes for lower office. To gauge the influence of coattails on the outcomes of elections, it is also necessary to consider the vote that the lower office would have received in the absence of a contest for the higher office. A virtue of analyzing coattail effects with data for all legislative election years is that this counterfactual quantity can be estimated. The dummy coefficients for the higher-office election years permit the total effects of the coattails—the products of the coattail coefficients and the respective higher-office votes—to be corrected for the level of Democratic legislative support that can be expected when coattails are not possible. The correction suggests that presidential candidates exert the greatest potential influence over state legislative outcomes.

This is evident in considering the thresholds that higher office support must exceed before coattails add to the Democratic share of seats that would result without higher offices on the ballot. For Democratic presidential candidates that threshold has been 32.9%; even weak national tickets have contributed something extra to the Democratic result in presidential years in the states.[7] For Democratic senatorial candidates, however, the threshold has been 49.7%, and for gubernatorial candidates 57.7%. If each of these candidates were to receive 50% of the vote, the total effect of presidential coattails would be a hefty 5.43%, of senatorial coattails a trivial .10%, and of gubernatorial coattails a *minus* 2.71%. In other words, presidential coattails are always likely to help the party's legislative cause, but senatorial and gubernatorial coattails will only help if the candidates are unusually popular. This helps to account for the midterm losses that the president's party invariably suffers in state legislative elections. The absence of presidential coattails in midterm elections and the weakness of compensating gubernatorial and senatorial coattails have cost the president's party 4.33% of lower-house seats in the off years.[8]

Contributing further to those losses has been the midterm decline in turnout. With typical turnout swings from presidential years to nonpresidential years in the range of 5–20%, and presidential voting swings in the range of 0–15%, the interaction between the two has averaged 111.9 in absolute value for the 22 elections and 35 states in this sample. When multiplied by the significant surge or decline coefficient of .008, changes in turnout (and its assumed composition) are estimated to have given the party of the victorious presidential candidate a legislative seat boost of .9% during presidential years and a penalty of the same magnitude in off years. This effect is only one-fourth that of the coattail effect, but it indicates nonetheless that presidential races influence legislative races at the state level in

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much the same way that they do at the national: by affecting not only the direction of voter choices but the number of people who show up at the polls to make them.

The final influence on state legislative races that is included in the core model is the national economy and the reward or blame that the president receives for its performance. Changes in the rate of real per capita disposable-income growth are estimated to exert a significant, independent effect on the share of state legislative seats captured by the president's party.[9] A one-point increase in the rate of real disposable-income growth during the year prior to an election increases the proportion of seats held by the president's party by .53%, an effect comparable in magnitude to those typically found in macroanalyses of congressional elections (see Rivers 1986). Because economic conditions are typically changing more favorably during presidential than during midterm election years—this measure averaged +.67% and was never negative during the former but averaged −.64% during the latter—they will also contribute to midterm seat losses by the president's party. On average, changes in real per capita disposable income nationwide have increased the president's party's control over a state legislature by .35 percentage points during presidential years and decreased it by .34 percentage points during off years.

These results indicate that state legislative elections are subject to much the same set of national influences as congressional elections. While coattails, especially those of the president, have exerted the largest average influence, economic conditions, when they slip or improve sharply, can cause significant legislative swings; and voter participation, where it oscillates severely, can induce a sizable measure of legislative instability too. From 1940 to 1982 these influences together were responsible for an average off-year loss by the president's party of 5.48 percentage points in the typical nonsouthern state legislature—a loss that may well be sufficient to account for the persistently large number of legislatures over which the president's party relinquishes control at midterm. This, of course, is of some importance because that loss has never been adequately explained. But the results may also hold importance for state government more broadly.

State Economic Conditions and State Legislative Elections

State government is widely believed to be influenced by state economic conditions. Political scientists have come to see state fiscal capacity as a serious constraint upon state policy-making (e.g., Jennings 1979). Economists, going a step further, have suggested that the constraint is even more severe, tightened by the competition among states for businesses and taxpayers (e.g., Oates 1972). This constraint, economists argue, has more profound implications for subnational government than political scientists generally acknowledge (but cf. Peterson 1981), for it pushes those governments away from policies that are anathema to rational taxpayers—for instance, steeply progressive taxation and aggressively redistributive spending—and toward others, such as economic development, that are more nearly Pareto-optimal. Yet, if state government is constrained, either severely or less so, by its economic circumstances, it is not entirely clear how that constraint operates.

Economists stress that citizens impose the constraint through their mobility—their willingness to "vote with their feet" (see, e.g., Tiebout 1956). But the evidence for that, though extensively investigated, is only mixed (Rose-Ackerman 1983). The other logical possibility is that the constraint is imposed through voting in the ordinary sense. This may be done di-

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Table 2. Annual Interstate Variances for State Economic Conditions, Averaged by Decade

Decade	Percentage Growth in Real Income[a]	Change in Real Income Growth[b]
1930s	63.03	117.44
1940s	40.24	63.87
1950s	12.03	36.57
1960s	11.02	22.97
1970s	6.70	21.28
1980s	4.15	8.11

Note: Data from each even-numbered year for 1932–82, inclusive.

[a]Annual percentage-point change in real per capita personal income by state.

[b]Annual change in the percentage-point growth of real per capita personal income by state.

rectly, as economists posit, with voters selecting among packages of burdens and benefits proposed by candidates for state offices. Or it may be done indirectly, as political scientists generally believe, through voter evaluations of government performance. If a state government pursues policies that are economically ill advised (in whatever sense), the state economy will suffer and the incumbent government will be turned out of office. In other words, state government may be held economically accountable through retrospective voting.

That hypothesis, which has not been extensively investigated and which enjoys more anecdotal than systematic support (cf. Jewell and Olson 1982; Turett 1971), is explored here by incorporating into the core model of state legislative elections a measure of state economic conditions: the annual change in real personal income by state.[10] This measure (see Table 2) reveals a great deal of variation in the year-to-year performances of state economies. Annual income growth rates, which fall primarily in the range of ±5%, have annual interstate variances (col. 1), even after the Great Depression, as high as 40%. The annual changes in those growth rates (col. 2), which are used in the election model, vary even more. While the variation in both has declined steadily over time—a point that will be addressed subsequently—there would appear to be sufficient differences in state economic conditions for voters to react to them, as well as to common national conditions.

There is, in any case, a potentially different line of political accountability for state conditions. In the core model the influence of the national economy was examined by assuming that it is the president and the president's party (including its state branches) who are held accountable for national economic performance. In this extension of that model the influence of the state economy will be assessed analoguously: by assuming that it is the governor and his or her party who are held accountable for state economic performance.[11] The effect of state economic conditions on state legislative elections is therefore specified as an interaction between the annual change in real per capita personal income by state and a dummy variable (+1 if Democrat, −1 if Republican) for the party of the governor. This variable was added to the core model, and the new model was estimated as before.

The results, reported in Table 3, provide little support for the hypothesis, however it is derived, that voters hold state politicians responsible for state eco-

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Table 3. Coefficient Estimates for Model of Control of Lower State Legislatures, Including Both National and State Economic Conditions, Non-South, 1940–82

Variable	Coefficient	*t*-score
Constant[a]	20.60	5.94*
Prior % Democratic seats[b]	.42	7.47*
% Democratic, president	.32	4.90*
Presidential election year	-10.39	3.10*
% Democratic, Senate	.33	6.89*
Senatorial election year	-16.07	6.49*
% Democratic, governor	.35	5.64*
Off-year % Democratic, governor	.08	1.83**
Gubernatorial election year	-20.23	6.78*
Surge or decline	.008	2.97*
Presidential responsibility for change in national income growth[c]	.54	4.83*
Gubernatorial responsibility for change in state income growth[d]	.07	1.37
Number of cases	751	
R^2	.75	
Adjusted R^2	.74	

Note: The dependent variable is the Democratic percentage of legislative seats by state.

[a]The variable list excludes dummies that were included for 34 states.

[b]The instrumental variables used to estimate this model include all exogenous variables and their two-year lags.

[c]Annual change in the percentage-point growth of national real per capita disposable income.

[d]Annual change in the percentage-point growth of real per capita personal income by state.

*$p \leq .05$, two-tail test.
**$p \leq .10$, two-tail test.

nomic performance. The coefficient on state economic conditions implies a shift of legislative seats of only .07% for every one-percentage-point change in real income, and even that comes with a standard error that is too large to distinguish the effect from 0. Putting aside its statistical significance, its substantive significance pales by comparison to national conditions, which are comparably measured: the effect of presidential performance in managing the national economy is more than eight times as large as the effect of gubernatorial performance in managing the state economy. The influence of state economic conditions is not being swamped, moreover, by the presence of national economic conditions. When state economic changes are the only economic conditions in the model, they remain small and nonsignificant.

The effects of the other variables in the model are essentially the same, whether state economic conditions—and gubernatorial responsibility for them—are included in the estimation or not. Again, state legislative elections appear to turn on factors that are substantially national. To be sure, gubernatorial coattails matter, and these may be influenced by the performance of the state economy. Turnout counts too, and it oscillates differently from state to state. But it is impressive how little state economic management—so central in economic conceptions of state government—seems to influence state legislative elections. This is not to say that politicians do not fear voter retribution for poor economic performance nor pursue policies that will avoid it. After all, they are blamed for those dips in all state economies (i.e., na-

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tional downturns) that occur while a president of their party is in office. But state politicians may have less to fear about the economic competitiveness of their states per se than is often thought; their elections do not much depend on it.

Institutionalization and State Legislative Elections

One reason why state legislative elections may not turn on state management of economic conditions is that state legislators do not want them to. Relative to the national government, state governments have little control over their economies. They have no access to monetary policy instruments and they are limited in their ability to make fiscal policy by constitutional requirements (at least in 49 of the states) that they balance their budgets. Over time, they have also seen the national economy—not to mention the international economy—become more integrated, as evidenced in Table 2, and in the process factors beyond their control gain in influence over their economic well-being. It may be true that a state's tax and expenditure policies will have a greater effect on its economic condition in a more homogeneous national economy than in one that is not well integrated—those policies representing the major marginal differences between state business climates. But because state politicians can only expect to benefit from such policies if their enactments are not countered by other states, the political benefits of trying to boost the state economy, even when state policies may be most consequential, are uncertain. Of course, wishing to remain blameless for state economic performance does not make one so. However, politicians have ways of protecting themselves against adverse swings in electoral conditions, and there are reasons to believe that state politicians have been seeking this protection—not only against the vicissitudes of state economies, but against all conditions over which they have limited control.

Traditionally, state legislators were not known for this sort of behavior. The archetypical state legislator was an amateur politician who served a term or two, then stepped down to return to private life (see, e.g., Hyneman 1938; Wahlke et al. 1962). Because they lacked the ambition to make politics a career, state legislators did not worry about protecting themselves against electoral tides. There was also little incentive to behave any differently. State legislatures were poorly paid, and the job occupied relatively little of their time, many legislators being in session for only several months every other year. Even if a legislator happened to find the position attractive, moreover, he or she had few resources, such as personal or professional staff or other perquisites of office, to help maintain it. State legislators were consequently not well insulated against electoral adversity—and state legislatures were not well institutionalized.

This is of considerable importance because institutionalization is generally regarded as a necessary condition for the effective functioning of a democratic organization (see, e.g., Converse 1969; Huntington 1965; Polsby 1968). Without it a democratic organization is prone to unresolved conflict, sudden changes in purpose, unrepresentative behavior, and problems of competence. In an institutionalized organization, members have a stake in its perpetuation, contending interests have incentives to compromise, and problems are more effectively solved through a permanent division of labor. The institution is more motivated to respond to constituent needs and is better organized to serve them.

Though once generally lacking in institutional capacity, and perhaps still seriously short of it (see, e.g., Houseman 1986), state legislatures have, over the last

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two decades, been adding capacity rapidly. Legislative sessions have been increased in length and frequency; committee and personal staffs have been expanded; ancillary research and support agencies have been created; and legislative salaries and perquisites have been increased (Sabato 1982). State legislators consequently have more incentives to try to maintain their positions—to protect themselves against electoral defeat—and superior means to do so. Like members of the U.S. House of Representatives, state legislators now preside over hundreds of domestic programs and have leverage, however limited, over an enormous bureaucracy, larger in total than that of the federal government (Chubb 1985). So situated, many state legislators have access to all of the tools of the incumbent's trade: casework, pork-barreling, credit claiming, and advertising. It follows that state legislators will be pursuing protective strategies, and that state legislatures should be becoming more institutionalized.

The electoral effects of these developments, however, remain rather unclear. It is known (see, e.g., Calvert 1979; Ray 1974, 1976; Rosenthal 1974; Shin and Jackson 1979) that member turnover in state legislatures declined, albeit unevenly, during the period 1940–80, as more incumbents chose to make the legislature a career. This follows, of course, from the increasing attractiveness of state legislative positions. But this is only a part, and in fact only an early part, of the process of institutionalization. Institutions automatically acquire stability when members decide that they value and want to remain in them. But that stability is a long way from that which institutions acquire when members find ways to protect their positions against outsiders—that is, electoral challengers—who also find those positions attractive and can bid for the support of voters. Legislatures do not become thoroughly institutionalized, therefore, when legislators decide they want to keep their positions but when they demonstrate that they can.

So viewed, the electoral dimension of institutionalization can be gauged by the parameters of the model of state legislative elections. If state legislators are succeeding in securing their positions, the control of state legislatures should prove increasingly unresponsive to forces over which legislators have no control, and increasingly dependent upon prior levels of party strength. To see if this is so, the model, including both national and state economic conditions, was estimated for two successive periods, 1940–66 and 1968–82. The breaking point, to some degree arbitrary, coincides with several developments that profoundly affected the institutionalization of U.S. government at the national as well as the state level: the weakening of political parties and mass partisanship, the ascendancy of entrepreneurial politics in congressional elections, and the expansion of state fiscal, administrative, and legislative capacities. At the national level these developments contributed to the atomization of congressional campaigns and the decentralization of Congress. At the state level they provided incentives and resources for development in the same direction. A comparison of elections on either side of this watershed (regardless, estimation indicates, of which year from the mid–1960s to the early 1970s is chosen to mark it) is a way of gauging the effects that these developments had on the sensitivity or vulnerability of state legislative elections to various electoral forces. It is, in other words, a way of measuring electoral insulation. The comparison made in Table 4 uses the coefficients of the extended legislative-election models, estimated as before.

Plainly, legislative elections give signs of institutionalization. To begin with, election outcomes in the more recent period are three times as dependent on the

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Table 4. Coefficient Estimates for Complete Model of Control of Lower State Legislatures, Non-South, Successive Periods 1940–66, 1968–82

Variable	1940–66		1968–82	
	Coefficient	*t*-score	Coefficient	*t*-score
Constant[a]	19.66	5.18*	13.45	1.91**
Prior % Democratic seats[b]	.23	3.39*	.73	7.06*
% Democratic, president	.52	5.89*	.13	1.25
Presidential election year	-20.64	4.19*	-7.01	1.48
% Democratic, Senate	.40	6.01*	.11	2.08*
Senatorial election year	-20.33	5.99*	-4.28	1.48
% Democratic, governor	.41	5.12*	.22	2.28*
Off-year % Democratic, governor	.08	1.38	-.10	1.40
Gubernatorial election year	-21.57	5.75*	-9.13	2.12*
Surge or decline	.002	.56	.014	4.57*
Presidential responsibility for change in national income growth[c]	.55	4.02*	.99	5.54*
Gubernatorial responsibility for change in state income growth[d]	.03	.57	.05	.66
Number of cases	468		282	
R^2	.82		.81	
Adjusted R^2	.80		.77	

Note: The dependent variable is the Democratic percentage of legislative seats by state.

[a]The variable list excludes dummies that were included for 34 states.

[b]The instrumental variables used to estimate this model include all exogenous variables and their two-year lags.

[c]Annual change in the percentage-point growth of national real per capita disposable income.

[d]Annual change in the percentage-point growth of real per capita personal income by state.

*$p \leq .05$, two-tail test.
**$p \leq .10$, two-tail test.

prior level of party support as they are in the earlier period, the coefficient of retention (.73) reaching nearly 75%. This in itself is a measure of increasing electoral stability, but more telling measures are those for the effects of outside electoral forces—which, overall, decline. Coattails, the strongest influence on legislative swings when all years (i.e., 1940–1982) are analyzed together, are an even stronger influence in the earlier period. Each higher office pays a dividend of nearly half a percentage point in seats for each percentage point in Democratic voting it attracts. Moreover, the thresholds of support that must be achieved before these offices exert their influence remain roughly the same.[12] In contrast, coattails in the more recent period are very short. Those of presidential and senatorial candidates are about one-third their earlier sizes, and those of gubernatorial candidates about half.

There are exceptions to this pattern. Legislators are not better protected against surges and declines in the size of the electorate (hence in the number of presidential supporters). In fact, they are more vulnerable to this phenomenon in the later period than in the earlier. This is simply an indication that presidential elections have not declined in influence over state elections as much as the coattail effect alone suggests. Legislators are also no less immune to the consequences of state economic performance—but not because

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those consequences have become more severe. To the contrary, state economic conditions had little influence on state elections in the forties, fifties, and sixties, and continued to have little influence in the years thereafter. Notwithstanding the considerable variation in state economic performance prior to the 1960s, there is no evidence that state legislatures were held accountable for it. But given the limited ability of state legislators to affect economic conditions within their states and the consequently weak incentives for them to seek responsibility for economic performance, this should not be entirely surprising.

It appears that when it comes to assigning responsibility for economic performance, state voters have generally and increasingly looked outside of the state—to the national economy and the president's imputed performance in managing it. Through the mid-1960s a one-percentage-point change in the rate of real per capita disposable-income growth changed the average state-legislative-seat share of the president's party by one half of a percentage point; after that the effect nearly doubled. This development is perfectly consistent with the steady integration of the national economy and the sustained strength that economic retrospective voting appears to have had at the national level (e.g., Fiorina 1981). But it stands as an important exception to the process of electoral insulation that is under way in state legislatures more generally. While, all things being equal, legislative elections exhibit greater overall stability, less sensitivity to higher-office influences, and continued invulnerability to state economic performance, they do show an increased susceptibility to national economic performance.

How do these changes add up? Despite the increased influence of the president's management of the nation's economy, state legislative elections have become substantially insulated against periodic swings, including those at midterm against the president. Based on the mean values of electoral forces during the respective periods—which do not differ themselves very greatly—the average estimated loss of seats for the president's party fell from 8.73% in the period 1940–66 to 3.34% in the period 1968–82. Evidently, the incentives and resources that state governments provided their legislators helped to insulate legislative elections against most periodic shocks.

This is confirmed in a comparative analysis of states that have made more and less progress in establishing conditions conducive to legislative reelection. A good proxy for these conditions—because it is highly correlated with other measures of institutional development, such as legislative professionalization (Citizen's Conference on State Legislatures 1971), and is itself a direct measure of reelection incentives—is total annual legislator compensation (or the sum of salary, benefits, and cash allowances). It was used along with state median family income to divide the states into two groups: states that were, toward the end of the period under study, offering legislative compensation equal to, or in excess of, median family income; and states that were not. In the higher-compensation states, the position of legislator was considered to be sufficiently attractive to provide career incentives—an individual could support a family on the income from the position alone—while in the lower-compensation states, it was not. Among the 35 states in this analysis, 10 fell into the higher-salary group and 25 into the lower.[13]

The question, then, is whether states that differ in their political incentives and resources also differ in their electoral dynamics. It appears that they do. The model was estimated as before (but excluding state economic conditions, which consistently proved nonsignificant) for each of the two groups. In higher-salary states (see Table 5) there is strong

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Table 5. Coefficient Estimates for Core Model of Control of Lower State Legislatures, Non-South, 1940–82, High and Low Legislative Salary States

Variable	High Salary States		Low Salary States	
	Coefficient	*t*-score	Coefficient	*t*-score
Constant[a]	13.95	2.36*	21.43	6.24*
Prior % Democratic seats[b]	.60	6.02*	.41	7.86*
% Democratic, president	.34	3.32*	.37	4.89*
Presidential election year	-8.50	1.42	-13.33	3.40*
% Democratic, Senate	.16	2.38*	.33	6.06*
Senatorial election year	-8.45	2.41*	-16.72	5.72*
% Democratic, governor	.28	2.39*	.40	5.81*
Off-year % Democratic, governor	.15	1.60	.06	1.10
Gubernatorial election year	-16.99	3.12*	-22.25	6.61*
Surge or decline	.008	1.74**	.009	2.50*
Presidential responsibility for change in national income growth[c]	.60	4.12*	.60	6.46*
Number of cases	234		619	
R^2	.74		.77	
Adjusted R^2	.72		.76	

Note: The dependent variable is the Democratic percentage of legislative seats by state.

[a]The variable list excludes dummies that were included for 34 states.

[b]The instrumental variables used to estimate this model include all exogenous variables and their two-year lags.

[c]Annual change in the percentage-point growth of national real per capita disposable income.

*$p \leq .05$, two-tail test.
**$p \leq .10$, two-tail test.

evidence of institutionalization; in the lower-salary states there is less. Where the incentives and resources for electoral protection are high, the rate at which the prior share of seats is normally maintained is 50% greater than where incentives and resources are low. In the poorer-paying states, most of the influences from which legislators would presumably like to be insulated are stronger than they are in the better-paying states—and none is significantly less influential. Electoral insulation and institutional development indeed appear to go hand-in-hand.

But this is not to suggest that institutionalization is taking place only in those states that have enriched their legislatures the most. A striking result of the comparison of well-endowed and not-so-well-endowed legislatures is that the differences in the coefficients between the respective models are not as large as might be expected: they are noticeably smaller than the differences (see Table 4) that were estimated for all states for successive periods. Electoral insulation evidently differs more between the past and the present than between the more and less developed states of today. In fact, when separate models were estimated for earlier and later years in the high- and low-salary states, the results indicated that electoral insulation is occurring across the board.[14] The insulation of state-legislative elections is not a widely scattered phenomenon. To be sure, it is more pronounced in some states than in others. But in virtually all states it appears to be establishing a level of electoral, and hence institutional, stability considerably higher than that for which state legislatures have traditionally been known.

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Institutional Capacity and Gubernatorial Elections

State executives, like state legislatures, were once subject to considerable criticism for their incapacity to govern effectively. Executive branches were badly fragmented, their many agencies often headed by independently elected chiefs. The governorship was hemmed in by various constitutional limits on its authority. The individuals who occupied the office were not infrequently political amateurs who lacked an effective base of political power. And this says nothing of the difficulties that governors inevitably faced in overseeing their economies. In recognition of these problems, efforts to reform state executives began to gain momentum shortly after World War II and became a regular part of state politics from the 1950s through the early 1970s. While these efforts addressed many problems, they often focused on gubernatorial weaknesses thought to derive from the circumstances of their election.

Certain gubernatorial election rules, it was argued, tended to undermine the political strength of the governor (Sabato 1982). Two-year terms, prohibitions against self-succession, and elections coinciding with elections for the presidency conspired to produce executive instability and ineffectiveness. Governors were often instant lame ducks, and if they were not emasculated by that status, their prospects for longevity were so uncertain, given the unpredictability of electoral conditions, that they had difficulty establishing a credible position of power while in office. For these reasons, many states reformed their governorships: the number of states providing four-year terms for governor increased from 23 in 1940 to 46 today; the number permitting self-succession (at least once) rose from 28 in the late 1950s to 46 in the 1980s; and the number electing governors in presidential years fell from 30 in 1956 to 13 in 1984. Coupled with real improvements in the salary, staff, and constitutional authority of state chief executives, changes in the governorship, accumulated throughout the postwar era, established a foundation for political independence and electoral strength in this key state office.

But how has all of this affected the election of governors? As in the case of state legislatures, there is considerable uncertainty. Presidential coattails have obviously become less important in states that moved their gubernatorial elections to off years, but their importance in those states that did not is unclear. While there is some evidence that incumbency provides governors little advantage, efforts to disentangle incumbency from other sources of gubernatorial support (e.g., Piereson 1977; Tompkins 1984; Turett 1971) have reached mixed conclusions about its past and current benefits. To be sure, there is agreement (see, e.g., Jewell and Olson 1982; Patterson 1982) that partisanship exerts a strong influence on gubernatorial elections, but even that result could benefit from a consideration of competing explanations and a larger set of cases. Finally, and this may be of the greatest importance for state government more broadly, the electoral accountability of governors for state economic performance has scarcely been investigated. Are strong state executives, like U.S. presidents, now held accountable for their economic showing?

A Model of Gubernatorial Elections

While state reformers have insulated gubernatorial elections from the direct influence of presidential coattails, there is ample evidence that they have not insulated them from presidential electoral forces more generally. Until 1986, the president's party had gone more than 60 years without increasing its share of

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Table 6. Coefficient Estimates for Gubernatorial Election Outcomes, 1940–82

Variable	Model with National Economic Conditions		Model with National and State Economic Conditions	
	Coefficient	*t*-score	Coefficient	*t*-score
Constant[a]	4.51	.43	2.64	.24
Prior % Democratic, governor[b]	.91	4.24*	.94	4.44*
Gubernatorial incumbency	-.59	.52	-.73	.63
% Democratic, president	.01	.15	.01	.16
Presidential election year	-.12	.03	-.11	.03
% Democratic, Senate	.12	2.49*	.12	2.26*
Senatorial election year	-6.36	2.39*	-5.86	2.12*
Surge or decline	.008	2.60*	.009	2.70*
Presidential responsibility for change in national income growth[c]	.32	3.75*	.32	3.68*
Gubernatorial responsibility for change in state income growth[d]			.07	1.79**
Number of cases	666		666	
R^2	.65		.64	
Adjusted R^2	.61		.61	

Note. The dependent variable is the Democratic percentage of the gubernatorial vote by state.

[a]The variable list excludes dummies that were included for 34 states.

[b]The instrumental variables used to estimate this model include all exogenous variables and their two-year lags.

[c]Annual change in the percentage-point growth of national real per capita disposable income.

[d]Annual change in the percentage-point growth of real per capita personal income by state.

*$p < .05$, two-tail test.
**$p < .10$, two-tail test.

governorships in a midterm election, and from 1950 to 1984 it lost an average of 6.2 in each one (Bibby 1983). This suggests, of course, that gubernatorial elections are subject to many of the same forces as national and, as we have seen, state-legislative elections. The model of gubernatorial election outcomes is therefore specified using the same potential influences as before.

The percentage Democratic for governor is modeled as a simple linear function of a baseline of support, given by the prior percentage Democratic for governor, and a number of forces expected to produce deviations from it: coattails, turnout, economic conditions, and incumbency. All variables are measured and specified as in the model of state legislatures except, of course, gubernatorial coattails, which are irrelevant and excluded, and gubernatorial incumbency, which is specified with a dummy variable, 1 if the candidate currently holds the office and 0 otherwise. The model was estimated as before, but for 45 rather than 35 states, for the same period, 1940–82. Only states that held gubernatorial elections in odd years were excluded. The results, including national economic conditions alone and then in conjunction with state economic conditions, are reported in Table 6.

Gubernatorial elections display important differences from state legislative elections. To begin with, they are less predictable. The model correctly "predicts" the winner of slightly less than three-fourths

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of the gubernatorial elections in this analysis while the comparable model gets the control of state legislatures right in more than 80% of the cases. The model also explains 15% less of the electoral variance. Gubernatorial elections, being high-visibility affairs, evidently attract attention to factors more idiosyncratic than those in the model—for instance, the personal attributes of the candidates—and, in consequence, are tougher to forecast. But this is not to say that gubernatorial elections are substantially unpredictable: they do have a core of stability. Much as previous research has suggested, gubernatorial elections have a strong general tendency to reproduce the level of support for the candidate of each party. All things being equal, gubernatorial candidates retain 90% of the support received by their respective parties' previous contestants.

Gubernatorial elections are also less influenced by systematic outside forces than are legislative elections. Presidential coattails, which are being structured out of existence through off-year gubernatorial elections, have a negligible impact when governors and presidents do run for election at the same time. Senatorial coattails are significant, but their impact on the Democratic percentage for governor is one-third their impact on the party's share of the state house. This is not to say that national-level forces are unimportant. Presidential voting in conjunction with oscillations in turnout—surge and decline—produces shifts in gubernatorial voting. And judgments about the president's performance in managing the national economy induce a smaller but still nontrivial response. Finally, state economic conditions, and the assumption of gubernatorial responsibility for them, have a significant impact on gubernatorial election outcomes. The impact, however, is small (one-fourth the impact of national conditions), and it does not increase in size when it is estimated in the absence of national economic conditions. If gubernatorial elections turn on economic performance, it is generally the performance of the president in managing the national economy that really matters.

Viewed together, these influences lead to several clear conclusions about gubernatorial elections. First, they are strongly influenced by party or, more specifically, the respective levels of support for the parties in the most recent gubernatorial election. Second, gubernatorial elections are regularly, though modestly, perturbed by national conditions. The aggregate average toll of those conditions on voting for the president's party in midterm gubernatorial elections is 1.9% per race, an amount that may well be sufficient, given the competitiveness of these contests, to account for quadrennial losses in governorships by the president. Third, these elections are affected more by national economic conditions and presidential responsibility for them than by state conditions and gubernatorial contributions to them. While state economic conditions do make a detectable difference—they raise the expected average midterm loss by the president's party to 2.2% per race—they indicate that state governors, like their legislatures, are not typically held accountable for state economic performance. Finally, gubernatorial elections display a healthy measure of unpredictability, the model "picking" the winner incorrectly over 25% of the time. While party establishes a firm baseline, and outside influences encourage a regular pattern of change, these elections can easily turn on the qualities of the candidates themselves, qualities that a macromodel can never adequately represent.

How much do these conclusions owe to the reforms of state governorships? That cannot be answered definitively with these estimates because reforms were being instituted over most of the period that has been analyzed. Unlike the reforms of state legislatures that occurred in most states over a fairly short period—that is, the last

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20 years—the reforms of state executives spanned the entire postwar era, occurring at different times in different states. It is therefore inappropriate to ask, as we did of state legislatures, whether their elections look different in recent years than in early years or in some states rather than others. There is no simple way to divide the states or the time period of this analysis into "reformed" and "unreformed" groups of cases. A more refined method of categorization, beyond the scope of this inquiry, is plainly required.[15]

Still, the results do not suggest that governors have been given effective control over their own fates. One major reform, the provision of the opportunity for self-succession, appears to have had little affect per se. Incumbency provides no advantage (the coefficient is nonsignificant) over and above that which redounds to any candidate who follows a popular governor of the same party onto the ticket. Another reform, off-year election scheduling, has largely freed governors from direct presidential coattails, but it has subjected them to midterm presidential punishment—meted out for poor national economic performance and manifest, in part, through declines in turnout—that governors cannot overcome by taking credit for their state's economic performance. In important respects, then, gubernatorial elections remain contests of party and personality and not of performance—at least not that of the governor and the state.

Discussion

State elections stand to shed considerable light on the recent progress of state governments because their outcomes are logically bound up with important influences on state policy-making—specifically, institutional development and economic accountability. Governments that through institutional development are able to insulate their elections to an appropriate degree against uncontrollable external perturbations and inevitable changes in economic conditions are more likely to acquire the capacity for effective and representative performance. The capacity of state government has long been suspect because of various institutional deficiencies and economic vulnerabilities. Recent institutional and economic development has eroded some of these suspicions, to be sure. But doubts persist as the states are asked to assume ever greater shares of responsibilities once held by the national government. The directions that these developments will take state government are inevitably unclear. Yet there is more uncertainty than is necessary, for research on state government, even in a fairly well-developed area such as electoral behavior, has simply not kept pace with them.

In this exploratory effort, I have tested out a different approach to the analysis of state elections—an approach inspired largely by analogous investigations of national elections—and taken an initial look at a number of obvious electoral indicators of state economic constraints and institutional limitations. The approach appears reasonably promising. It does have some distance to go before it offers a full account of gubernatorial elections, but any macro explanation is bound to be confounded by the apparent role of personalities in those contests. In any event, it appears to be a very useful means of evaluating explanations of lower-visibility contests such as those for the state legislature. And it offers some telling insights into common assumptions about state government.

To begin with, state elections do not turn appreciably on the performance of the state government in managing its economy. This result at least calls into question the motivating assumption of models of interstate competition. If there is little evidence that state politicians are blamed or rewarded according to their

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performances as economic managers, there is reason to question how substantially those politicians are guided by the belief that they will be. Voters in state elections appear to hold politicians outside of the state, specifically the president and the president's party, responsible for economic conditions and, if conditions in a state should differ from those in neighboring states, to hold the governor only minimally responsible. Obviously, a good deal of research remains to be done on these matters; among other things, alternative specifications of state economic performance—for instance, changes in tax and expenditure policies—merit attention. But it stands to reason that in an increasingly homogeneous national economy, state governments, lacking as they are in economic-policy leverage, would not invite and could arguably deflect responsibility for state economic performance.

There is one other important respect in which state elections confound traditional suspicions. Legislative elections are becoming noticeably well insulated against political adversity—and in a way that has clear implications for the institutional capacity of state governments. Elections to state legislatures, while systematically affected by all of the dominant forces in U.S. elections (coattails, turnout, and economic conditions) have grown fairly immune to all of them (except the national economy) and, in the process, acquired substantial stability. Because these developments were especially pronounced in states that increased the resources of their legislators, there is good reason to believe that electoral insulation developed as part of a more general process of legislative institutionalization. That is important, for it suggests that as states provide legislators incentives and resources to maintain their positions, they can overcome a traditional incapacity of their governments.

Notes

The first draft of this paper was presented at the 1986 Annual Meeting of the American Political Science Association, and a revised version appeared as a Brookings Discussion Paper in Governmental Studies. The comments, criticisms and assistance of Steven Smith, Paul Peterson, Clifford Winston, Joe White, and Paul Wright were invaluable.

1. It would be preferable to break the relationship between coattail voting and the composition of legislatures into two components: (1) the effect of votes for a presidential candidate on votes for state candidates of the same party and (2) the effect of votes for those state candidates on the party's share of seats in the legislature. This is done for the U.S. Congress by Ferejohn and Calvert (1984). It cannot generally be done for state legislative elections because historical voting returns are not sufficiently available, especially for the years prior to 1970 (cf. Tidmarch, Lonergan, and Sciortino 1986).

2. Voting for the U.S. House of Representatives may also influence voting in individual state-legislative races. But since this analysis is aggregated to the state level, it could easily be misleading to use the average Democratic vote across all House elections in a state as a measure of any particular coattail effect.

3. Because data on turnout for state-legislative elections are not available for most years and most states in the analysis, a surrogate, turnout for U.S. House elections, is employed.

4. The data for this analysis were obtained from the Inter-University Consortium for Political and Social Research (State-Level Congressional, Gubernatorial, and Senatorial Data for the United States, 1824-1972; and Partisan Division of American State Governments, 1834-1984); *Statistical Abstract of the United States*, 1976-84; *Congressional Quarterly Almanac*, 1975-83; *Historical Statistics of the United States: Colonial Times to 1970*; and *Economic Report of the President*, 1975. The states excluded from the legislative analysis are Alabama, Florida, Georgia, Kentucky, Maryland, Minnesota, Mississippi, Nebraska, New Jersey, North Carolina, South Carolina, Texas, and Virginia.

5. The effects of the explanatory variables—coattails, turnout, and the economy—may also vary across the states (on coattails, see, e.g., Campbell 1986). But since there is no reason to expect this variation to be systematic, it will not bias the estimates of the slopes that are specified in this model to apply to all states.

6. Strictly speaking, the estimation procedure remains inefficient because serial correlation within states and heteroskedasticity across them were not corrected. But the inefficiency is likely to be slight: the lagged endogenous term in the model picks up

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much of the autodependence in the variables, the multiple state intercepts tend to reduce autocorrelation and heteroskedasticity, and a trend variable, year, included in the preliminary estimation of the model proved nonsignificant. Whatever serial correlation remains in the model is therefore unlikely to distort the standard errors or the tests of statistical significance appreciably—and, of course, the coefficient estimates remain consistent. Moreover, further improvements in efficiency that might be obtained by reducing autocorrelation or heteroskedasticity are best pursued using consistent covariance matrix estimation methods (Newey and West n.d.; White 1980) that have not yet been adapted for pooled applications. The methods that are available, such as generalized difference models that correct each state's error structure individually (Pindyck and Rubinfeld 1981), are inordinately cumbersome and provide dubious improvements.

7. The total coattail effect is given by the formula, (coattail coefficient) (vote for higher office) − (coefficient of dummy variable indicating years when the higher office is contested). Because the dummy variable coefficient is negative, the coattail effect does not become positive until the product in the formula exceeds the dummy variable coefficient. The threshold that the higher-office vote must cross to provide positive coattail effects is therefore obtained by dividing the dummy-variable coefficient by the coattail coefficient and multiplying by −1. For example, the threshold for positive coattails for presidential voting is given by the expression, $-1(-10.52/.32) = 32.9\%$.

8. The average percentage Democratic for president over this period was 46.42%; hence, the average effect of presidential coattails on state legislative seat shares was $(.32)(46.42\%) - (10.52) = 4.33\%$. In midterm elections the absence of presidential coattails cost the president's party the same amount.

9. Experimentation with other specifications of economic conditions indicated that this specification was robust. The coefficient on the change in the rate of real disposable-income growth maintained its strength when other economic conditions—specifically, changes in the inflation rate and in the unemployment rate—were included in the model. The coefficients of the other economic conditions, while always of the correct sign, varied in magnitude and significance, and were therefore excluded from the final specification.

10. Data on disposable income by state are not available back to 1940, so personal income is used instead. Evidence that the latter is a good proxy for the former, at least for this sort of analysis, comes from using it instead of disposable income as an indicator of presidential economic performance. Its estimated effect is very close in magnitude to that reported in Table 1 for disposable income.

11. It is also possible to assume that the president and his party are held responsible for state economic conditions. When tested, however, it proved significant only when presidential responsibility for national conditions was omitted from the model. When presidential responsibility for both national and state conditions were specified in the model, national conditions invariably proved to be the better gauge of electorally significant evaluations of presidential performance.

12. The presidential threshold is 39.5%, the senatorial 51.1%, and the gubernatorial 53.1%.

13. The salary and family-income data are from 1979. The higher-salary states are: Alaska, California, Illinois, Massachusetts, Michigan, New York, Oklahoma, Ohio, Pennsylvania, and Wisconsin. The lower-salary states are all of the rest except those excluded from the analysis (see n. 4). Several states fell just short of the higher-salary category and may, from a behavioral standpoint, be misclassified. But the purpose of this part of the analysis is not to model the electoral dynamics of particular states (which data limitations severely constrain anyway); the purpose is to gauge the electoral consequences of institutional maturation, for which legislative salaries are but a proxy.

14. In both groups of states the coefficient on the prior percentage Democratic increased by roughly the same amount, and the coefficients on the coattail variables dropped by approximately the same amounts.

15. What may well be required is a state-by-state specification of policy interventions, taking advantage of pooling to estimate multiple interrupted time series simultaneously.

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John E. Chubb is a Senior Fellow in the Governmental Studies Program, The Brookings Institution, Washington, DC 20036.

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NUCLEAR BRINKMANSHIP WITH TWO-SIDED INCOMPLETE INFORMATION

ROBERT POWELL
Harvard University

A brinkmanship crisis with two-sided incomplete information is modeled as a game of sequential bargaining in which each state is uncertain of its adversary's resolve. The sequential crisis equilibria are characterized explicitly and used to analyze the influences of resolve, misperception, and the status quo on escalation and crisis stability. The description of brinkmanship as a contest of resolve is found to be misleading: the state with the greatest resolve may not prevail in the crisis; a state may be less, not more, likely to prevail the greater its resolve; and a states' expected payoff may be less, not more, the greater its resolve. Moreover, reducing misperception may destabilize a crisis. Surprisingly, increasing the stake a potential challenger has in the status quo may not make a challenge less likely. Finally, crises involving severe conflicts of interest are shown to be less likely than crises not entailing a severe conflict of interest.

Recent work on crisis bargaining has studied nuclear brinkmanship when there is one-sided incomplete information in which only the potential challenger is uncertain of the type of its adversary (Powell 1987). I here extend the analysis of nuclear brinkmanship to the case in which there is two-sided incomplete information. A brinkmanship crisis is modeled as a game of sequential bargaining in which each state is uncertain of its adversary's resolve. The game is then solved for its sequential equilibria. Two-sided incomplete information is a more natural setting and allows for a richer bargaining dynamic.

With one-sided incomplete information, it was found that contrary to the implication suggested by the analyses of Ellsberg (1959), Jervis (1972, 1978), Snyder (1971, 1972), and Snyder and Diesing (1977), an increase in an adversary's resolve may make a state more, not less, likely to escalate. Moreover, the state with the greatest resolve may not prevail. Both findings continue to hold with two-sided incomplete information. But the richer dynamic also shows that the effects of an increase in an adversary's resolve may vary with the stage of the crisis. The greater an adversary's resolve, the less likely an irresolute potential challenger may be to challenge the status quo. But if it does dispute the status quo, it is more likely to escalate if its challenge is resisted.

The richer dynamic also indicates that describing a brinkmanship crisis as a contest of resolve (e.g., Jervis 1979a, 1979b; Schelling 1966) is misleading. This description would seem to suggest that the state with the greatest resolve will prevail; that the greater a state's resolve, the more likely it is to prevail; that the greater a state's resolve, the greater its expected payoff; or that it should at least be possible to describe the states' strategies as functions of the states' levels of resolve. In the model, all of these assertions prove incorrect.

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In the model the situation actually facing a state can be distinguished from the situation that the state believes itself to be facing. The degree to which this belief differs from the actual situation provides a measure of misperception and may be used to explore some of the consequences of misperception. The result is that although there would be no crises if misperception could be completely eliminated, reductions in the level of misperception may be destabilizing as well as stabilizing.

The value that a potential challenger places on maintaining the status quo plays a surprising role in crisis bargaining. Detente is at least in part based on the general belief that increasing an adversary's stake in the status quo makes a challenge to the status quo less likely, because disputing a more valuable status quo puts more at risk. This is a belief about foreign policy, but it has an obvious parallel in crisis bargaining. In the model, an increase in the value of the status quo does make a crisis less likely. But at least for small increases in the value of the status quo, the potential challenger is not less likely to challenge the status quo. Rather, the challenger's adversary is more likely to submit if challenged. Apparently, being known to have a greater stake in the status quo may be used as a bargaining lever by a potential challenger to convey an impression of greater resolve to its adversary, thereby making the adversary more likely to back down.

The analysis also implies that some types of crises are more likely than other types. If there is a crisis, then, perhaps surprisingly, the challenger is more likely to be irresolute than resolute. This implies that crises in which there is a severe conflict of interest are less likely than crises in which there is not a severe conflict of interest.

I first develop the model and then present the sequential crisis equilibria (which are formally derived in the Appendix). The effects of resolve, misperception, and the status quo on escalation and crisis stability are then considered. Finally the distribution of types of crises is examined.

The Model

If two adversaries possess secure second-strike forces and the requisite command and control capabilities, neither state can credibly threaten to launch a massive unlimited nuclear attack against the other during a crisis. Launching such an attack would bring one's own destruction. Whenever the cost of imposing a sanction, a massive nuclear attack in this case, is more costly than not imposing it, one cannot credibly threaten to invoke the sanction deliberately. Thomas Schelling (1960, 199–201; 1966, 92–125) has used the analogy of brinkmanship to explore the bargaining dynamics of this type of nuclear crisis. The model analyzed below formalizes and simplifies this analogy.

In Schelling's analogy, two adversaries are roped together and standing near a brink. The rope ensures that if one goes over the brink, so will the other. In the confrontation, each adversary tries to coerce the other into submission by raising to an intolerably high level the risk that they will tumble into the chasm.

Brinkmanship captures the essence of the credibility problem inherent in a situation in which imposing the sanction is more costly to oneself than not imposing it. In the analogy, neither party can credibly threaten to jump into the abyss deliberately. Thus, as long as the adversaries are in complete control of whether or not they go over the brink, there is no danger of going over and consequently no way to use the fear of going over to exert coercive pressure.

Schelling's solution to the credibility problem is "the strategy that leaves something to chance." For him, the brink is not

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a sharp edge that one can approach safely before deciding whether or not to jump. The brink is rather "a curved slope that one can stand on with some risk of slipping, the slope gets steeper and the risk of slipping greater as one moves toward the chasm" (1960, 199).

Describing the brink in this way means that one could go over the brink despite both adversaries' unwillingness to take any action that was certain to force them into the abyss. With gusty winds overhead and loose gravel underfoot, the parties are not fully in control of events. Accordingly, each can exert coercive pressure on the other by approaching the chasm and thereby creating a risk that despite one's desire not to slip, one still might slip accidentally. Assuming that neither does slip, then as both near the brink one of them would sooner or later find the risk intolerable and would submit. In this way, nuclear crises become a competition in taking risks.

Before turning to the model, two aspects of this analogy deserve special emphasis because they help to define the kind of crisis for which brinkmanship may be an appropriate analogy. First, there is only one sanction in the analogy and only one means of coercion. The single sanction is going over the brink, and this corresponds to an unlimited nuclear exchange in a crisis. The single means of coercion is manipulating the risk of suffering the sanction. Accordingly, options exercised during a crisis are not to be judged primarily in terms of their effect on the balance of forces but in terms of their effect on the risk of an unlimited nuclear war (Jervis 1979b, 1984; Schelling 1960, 190–94; Schelling 1962; Schelling 1966, 106–16). In actual crises, however, there may be other means of exerting coercive pressure. One of these means is the use of limited attacks intended to inflict pain and to make the threat of future punishment more credible.[1] Another means is the use of options designed to enhance one's relative military strength. The more important the role that these other means of coercion play in actual crises, the less appropriate is the analogy of brinkmanship.

The second aspect of the analogy to be emphasized is the crucial part that accident plays. Throughout the confrontation at the brink, each participant always prefers any other outcome to that of sliding into the chasm. There are no circumstances in which one would decide to jump off the brink deliberately, and this is common knowledge. By analogy, then, there are no circumstances in which a state would deliberately launch an unrestricted attack, even preemptively. In the brinkmanship analogy, Schelling's "reciprocal fear of surprise attack" (1960, 207–29) plays no role. But if the possibility of falling over the brink is to have any influence at all, there must be some chance of this happening. Since no party can credibly threaten to go over the brink deliberately, it must be possible for the sanction of an unlimited nuclear exchange to be the direct result of an accident. The analogy of brinkmanship places a heavy burden on accident.

Powell (1987) has recently examined the escalatory dynamics of a model of crisis bargaining with incomplete information. The model is presented as an extension of existing models of crisis bargaining in that it explicitly models two essential features of a crisis: a sequence of decisions and the lack of complete information. That model may also be seen as a formalization of Schelling's brinkmanship analogy in which there is only one-sided incomplete information. I will extend that model to include two-sided incomplete information.

Play begins with a potential challenger, I, deciding whether to accept the status quo; dispute the status quo by challenging its adversary, II; or jump off the brink deliberately by launching an unrestricted nuclear attack. If I accepts the status quo,

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the game ends with the payoffs (q_I, q_{II}). Attacking also ends the game and brings the disastrous payoffs (d_I, d_{II}). If I challenges II, then the onus of escalation shifts to II.

II now has three options, submitting, attacking, or escalating by moving toward the brink. Attacking again ends the game with payoffs (d_I, d_{II}). Submission ends the game with payoffs (w_I, s_{II}). The model simplifies the brinkmanship analogy by assuming that the states can move toward the brink in only one way, by increasing the risk of "slipping" by a fixed increment f. Thus if II escalates, it does so by generating a probability of disaster f. This is modeled by letting Nature move after II escalates. At this move, Nature imposes an "accidental" disaster with probability f. Going over the brink, whether accidentally or deliberately, brings a payoff of (d_I, d_{II}). If there is no disaster, the onus of escalation shifts back to I.

I must now decide if it will submit, attack, or escalate. Attacking, as always, ends the game with payoffs (d_I, d_{II}). I's submission ends the game with payoffs (s_I, w_{II}). If I escalates, it must do so by generating a risk of falling over the brink of $2f$. Nature then moves by playing disaster with probability $2f$. If there is no disaster, the onus of escalation shifts back to II who must now create a risk of $3f$ if it decides to escalate. The game continues in this way until one state submits or attacks or until there is a disaster.

For each state, the game can end in only one of four ways. The state can prevail, submit, continue with the status quo, or suffer a disaster. Since prevailing is better than submitting and submitting is better than disaster, $w_I > s_I > d_I$ and $w_{II} > s_{II} > d_{II}$. The status quo is also assumed to be better than submitting: $q_I > s_I$ and $q_{II} > s_{II}$. For I, prevailing is better than the status quo: $w_I > q_I$. Otherwise, I could obtain its highest payoff by accepting the status quo at the beginning of the game. There would never be a crisis, for I would never challenge the status quo. The relation between w_{II} and q_{II} is uncertain but also irrelevant since the status quo has already been violated by the time that II makes its first decision.

Resolve plays a crucial role in brinkmanship crises. A state's resolve is generally described as the maximum risk of disaster that a state is willing to run in order to prevail (Brams 1985; Jervis 1972; Powell 1987; Snyder 1972; Snyder and Diesing 1977; Wagner 1982). Because in the model a state can submit at any time during the crisis, the maximum risk of disaster that it would be willing to run in order to prevail is the level of risk that makes a state indifferent between submitting and gambling on prevailing. Letting R denote a state's resolve, then $(1 - R)w + Rd = s$ and $R = (w - s)/(w - d)$ where the subscripts have been dropped because the formulae apply to each state.[2]

A state's level of resolve determines an upper bound on the number of times that a state will escalate. To see this, note that the pattern of risks created during the crisis is $f, 2f, 3f, \ldots Kf$ where $Kf = 1$ and K is an integer. As will become clear, it makes no difference whether K is odd or even, so take K to be even. It is, however, important to ensure that each state is physically able to escalate at least twice in the game. The means $4f < 1$. Since II is the first to generate any risk, the risks it creates as it moves toward the brink follow the pattern $f, 3f, \ldots (K-1)f$. But II's resolve, R_{II}, is the greatest risk that II is willing to run, so the maximum number of times that II might be willing to escalate is the largest integer n satisfying $(2n - 1)f \leqslant R_{II}$. Let N_{II} be this integer. Similarly, the maximum number of times that a challenger might escalate by generating some risk of disaster, N_I, is the largest integer n that satisfies $2nf \leqslant R_I$.

With two-sided incomplete information, each state is uncertain of the type of its adversary. To simplify matters, there

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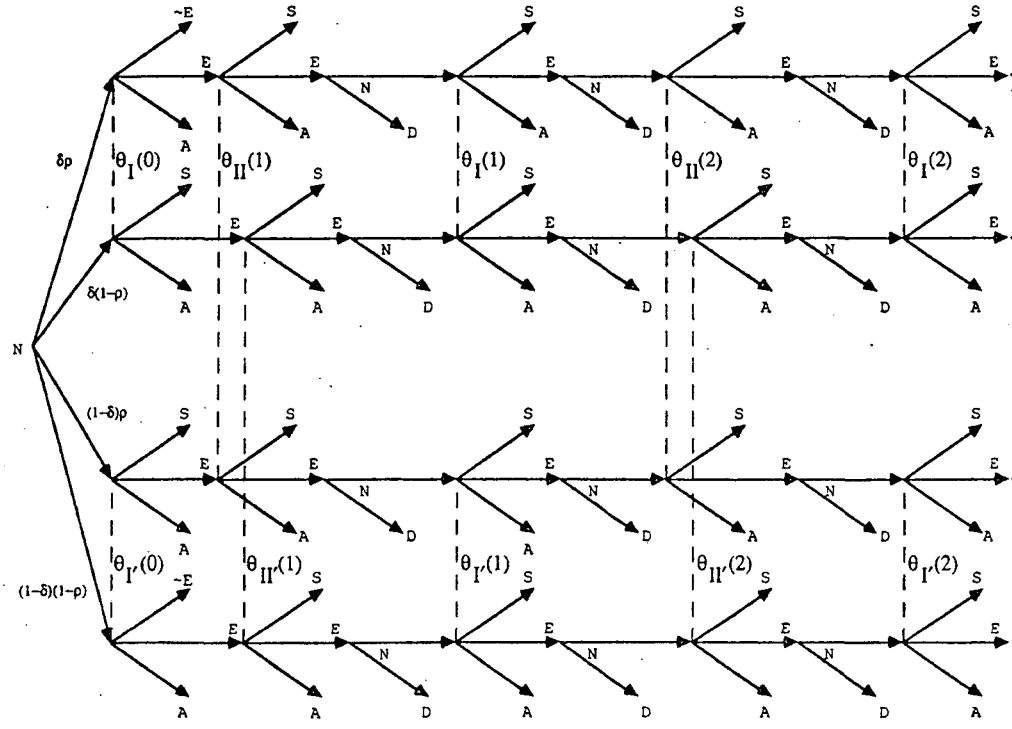
Figure 1. Nuclear Brinkmanship with Two-Sided Incomplete Information



are only two possible types of potential challenger, an irresolute and a resolute challenger. Let I denote the irresolute challenger and take R_{I} to be such that $N_{\mathrm{I}} = 1$, that is, $2f \leqslant R_{\mathrm{I}} \leqslant 4f$. To avoid some future technicalities, strict inequality will be assumed: $2f < R_{\mathrm{I}} < 4f$. The resolve of the resolute challenger, I′, is such that $N_{\mathrm{I}'} \geqslant 2$ which means $4f < R_{\mathrm{I}'}$. There are also only two possible types of challenged state. Again, one of these is resolute and the other is irresolute. II is irresolute with $f < R_{\mathrm{II}} < 3f$ so that $N_{\mathrm{II}} = 1$, and II′ is resolute with $3f < R_{\mathrm{II}'} < 5f$ so that $N_{\mathrm{II}'} = 2$. Finally, the probability of a potential challenger being irresolute is δ and the probability that its adversary is irresolute is ϱ. These probabilities are common knowledge.

The game's extensive form is depicted in Figure 1. The game begins with Nature selecting the types of the states. After these have been determined, play proceeds as described above with the onus of escalation shifting back and forth until the game ends. The options of *not exploiting* the situation, *exploiting* (or *escalating*), *attacking*, *submitting*, or, for Nature, imposing *disaster* are denoted by $\sim E$, E, A, S, D, respectively.

It will prove convenient to adopt the following notation for the states' information sets and behavioral strategies: Let $\theta_{\mathrm{II}}(n)$ be the information set at which II must decide whether or not to escalate for the nth time where $1 \leqslant n \leqslant K/2$. Thus $\theta_{\mathrm{II}}(n)$ presents II with the choice of submitting, attacking, or escalating by creating an autonomous risk of $(2n - 1)f$. (Recall that since the onus of escalation shifts back and forth, II only makes bids of odd multiples of f.) $\theta_{\mathrm{II}'}(n)$ is defined in exactly

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the same way for II′. For I, take $\theta_{I}(n)$ for $1 \leqslant n \leqslant K/2$ to be the information set at which I must decide to submit, attack, or escalate with probability $2nf$. $\theta_{II}(0)$ is the information set at which I has to choose among attacking, exploiting the situation, or not exploiting it. $\theta_{II'}(n)$ is defined similarly for I′.

A similar notation will be employed for a state's beliefs about the type of adversary it is facing at any given information set. Take an arbitrary information set, say $\theta_{II}(n)$. Then, II believes that it is facing an irresolute adversary at $\theta_{II}(n)$ with probability $\beta_{II}(n)$ and a resolute adversary with probability $1 - \beta_{II}(n)$.

At each of its information sets, a state has a choice among three alternatives. Accordingly, a behavioral strategy for any state at any information set is defined by two probabilities. At, say, $\theta_{II}(n)$, II's strategy may be described by the probability of attacking at that set, $a_{II}(n)$, and the probability of escalating, $e_{II}(n)$. The probability of submission is given by $1 - a_{II}(n) - e_{II}(n)$.

The Sequential Crisis Equilibria

This section presents the game's sequential crisis equilibria. In a sequential equilibrium, a state's equilibrium strategy starting from any place in the game is an optimal strategy for the rest of the game given the state's beliefs and given the other states' strategies. That is, no state can improve its payoff by deviating from its equilibrium strategy anywhere in the game.[3] Thus in a sequential equilibrium no state can rely on a threat to impose a sanction when actually imposing the sanction is expected to be more costly than not doing so. In this way, sequential equilibria avoid the credibility problems inherent in doctrines like massive retaliation.

For there to be a crisis, there must be a challenge and it must be resisted (Snyder and Diesing 1977, 13). Accordingly, this analysis will focus only on equilibria in which there is some chance of a resisted challenge. Formally, it must be that $e_{I}(0) > 0$ or $e_{I'}(0) > 0$ and that $e_{II}(1) > 0$ or $e_{II'}(1) > 0$. Equilibria satisfying these conditions will be called crisis equilibria.

Before characterizing the game's sequential crisis equilibria, two remarks are in order. First, note that no state will ever place a positive probability on attacking in a sequential equilibrium. If it did, then this state could improve its payoff by deviating from its strategy by submitting rather than attacking. This, however, is inconsistent with the definition of a sequential equilibrium. Thus, a state's strategy at any information set can be characterized by the single probability of the state escalating at this set.

The second remark simplifies the model further and facilitates the search for the game's equilibria by evading some of the difficulties of deciding what are reasonable conjectures to hold off the equilibrium path. The simplification is to ensure that the resolve of I′ is so great that it is certain to escalate at $\theta_{I'}(0)$ and $\theta_{I'}(1)$ regardless of what its adversary does. This assumption further constrains $N_{I'}$, which has already been assumed to satisfy $N_{I'} \geqslant 2$. This constraint will be specified when the game's equilibria are derived. For now, simply assume $e_{I'}(0) = 1$ and $e_{I'}(1) = 1$.

PROPOSITION 1. *In the game with two-sided incomplete information, there are four generic types of potential sequential crisis equilibrium. Generically, only one of these equilibria can exist at a time and which one, if any,*[4] *exists depends on the initial beliefs of the states, that is, on the values of* δ *and* ϱ.

Equilibrum 1 exists only if

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$$\delta > 1 - \left(\frac{R_{II'} - f}{(1-f)[(1-2f)R_{II'} + 2f]}\right)\left(\frac{R_{II'} - 3f}{(1-3f)[(1-4f)R_{II'} + 4f]}\right)$$

$$\varrho < 1 - \left(\frac{w_I - q_I}{w_I - d_I}\right)\frac{1}{(1-f)R_I + f}$$

with strategies

$$e^*_I(0) = \left(\frac{1-\delta}{\delta}\right)\left(\frac{1}{1-\beta^*_{II'}(2)}\right)\left(\frac{[1-(1-f)(1-2f))](1-R_{II'})}{R_{II'} - f} + \beta^*_{II'}(2)\right)$$

$$e^*_I(1) = [\beta^*_{II'}(2)(R_{II'} - f)/\{[1-(1-f)(1-2f)(1-R_{II'}) + \beta^*_{II'}(2)(R_{II} - f)\}$$

$$e^*_I(n) = 0 \quad \forall n \geqslant 2$$

$$e^*_{II}(n) = 0 \quad \forall n \geqslant 1$$

$$e^*_{II'}(1) = \left(\frac{1}{1-\varrho}\right)\left(\frac{w_I - q_I}{w_I - d_I}\right)\frac{1}{(1-f)R_I + f}$$

$$e^*_{II'}(2) = \frac{R_I - 2f}{(1-2f)[(1-3f)R_I + 3f]}$$

$$e^*_{II'}(n) = 0 \quad \forall n \geqslant 3$$

$$e^*_{I'}(n) = 1 \quad \forall n \leqslant N_{I'}$$

$$e^*_{I'}(n) = 0 \quad \forall n > N_{I'}$$

and with the relevant[5] beliefs

$$\beta^*_I(0) = \beta^*_{I'}(0) = \varrho$$

$$\beta^*_I(1) = \beta^*_{I'}(1) = 0$$

$$\beta^*_I(2) = \beta^*_{I'}(2) = 0$$

$$\beta^*_{II}(1) = \beta^*_{II'}(1) = [\delta e^*_I(0)]/[1 - \delta(1 - e^*_I(0))]$$

$$\beta^*_{II}(2) = \beta^*_{II'}(2) = \frac{[1-(1-3f)(1-4f)](1-R_{II'})}{(1-3f)[(1-4f)R_{II'} + 4f]}$$

Equilibrium 2 exists only if

$$\delta > 1 - \left(\frac{R_{II} - f}{(1-f)[(1-2f)R_{II} + 2f]}\right)\left(\frac{R_{II'} - 3f}{(1-3f)[(1-4f)R_{II'} + 4f]}\right)$$

$$1 - [(w_I - q_I)/(w_I - d_I)]\,[(R_I - 2f)/\{(1-2f)\,[(1-3f)R_I + 3f]\}]\,\{1/[1-f)R_I + f]\} > \varrho > 1 - [(w_I - q_I)/(w_I - d_I)]\,\{1/[(1-f)R_I + f]\}$$

with strategies

$$e^*_I(0) = \frac{1-\delta}{\delta} \cdot \frac{1}{1-\beta^*_{II'}(2)}\left(\frac{[1-(1-f)(1-2f)](1-R_{II})}{R_{II} - f} + \beta^*_{II'}(2)\right)$$

$$e^*_I(1) = [\beta^*_{II'}(2)(R_{II} - f)]/\{[1-(1-f)(1-2f)](1-R_{II}) + \beta^*_{II'}(2)(R_{II} - f)\}$$

$$e^*_I(n) = 0 \quad \forall n \geqslant 2$$

$$e^*_{II}(1) = 1 - \frac{1}{\varrho}\left[1 - \left(\frac{w_I - q_I}{w_I - d_I}\right)\frac{1}{(1-f)R_I + f}\right]$$

$$e^*_{II}(n) = 0 \quad \forall n \geqslant 2$$

$$e^*_{II'}(1) = 1$$

$$e^*_{II'}(2) = \left(\frac{1}{1-\varrho}\right)\left(\frac{w_I - q_I}{w_I - d_I}\right)\left(\frac{1}{(1-f)R_I + f}\right)\left(\frac{R_I - 2f}{(1-2f)[(1-3f)R_I + 3f]}\right)$$

$$e^*_{II'}(n) = 0 \quad \forall n > 2$$

$$e^*_{I'}(n) = 1 \quad \forall n \leqslant N_{I'}$$

$$e^*_{I'}(n) = 0 \quad \forall n > N_{I'}$$

and with the relevant beliefs

$$\beta^*_I(0) = \beta^*_{I'}(0) = \varrho$$

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$$\beta^*_{\mathrm{I}}(1) = \beta^*_{\mathrm{I'}}(1) = 1 - (1 - \varrho)\left(\frac{w_{\mathrm{I}} - q_{\mathrm{I}}}{w_{\mathrm{I}} - d_{\mathrm{I}}}\right)\frac{1}{(1 - f)R_{\mathrm{I}} + f}$$

$$\beta^*_{\mathrm{I}}(2) = \beta^*_{\mathrm{I'}}(2) = 0$$

$$\beta^*_{\mathrm{II}}(1) = \beta^*_{\mathrm{II'}}(1) = \{[1 - (1 - f)(1 - 2f)](1 - R_{\mathrm{II}}) + \beta^*_{\mathrm{II'}}(2)(R_{\mathrm{II}} - f)\}/\{(1 - f)[(1 - 2f)R_{\mathrm{II}} + 2f]\}$$

$$\beta^*_{\mathrm{II}}(2) = \beta^*_{\mathrm{II'}}(2) = \frac{[1 - (1 - 3f)(1 - 4f)](1 - R_{\mathrm{II'}})}{(1 - 3f)[(1 - 4f)R_{\mathrm{II'}} + 4f]}$$

$$\beta^*_{\mathrm{II'}}(3) = \beta^*_{\mathrm{II'}}(3) = 0$$

Equilibrium 3 exists only if

$$\delta > 1 - \left(\frac{R_{\mathrm{II}} - f}{(1 - f)[(1 - 2f)R_{\mathrm{II}} + 2f]}\right)\left(\frac{R_{\mathrm{II'}} - 3f}{(1 - 3f)[(1 - 4f)R_{\mathrm{II'}} + 4f]}\right)$$

$$\varrho > 1 - \left(\frac{w_{\mathrm{I}} - q_{\mathrm{I}}}{w_{\mathrm{I}} - d_{\mathrm{I}}}\right)\left(\frac{R_{\mathrm{I}} - 2f}{(1 - 2f)[(1 - 3f)R_{\mathrm{I}} + 3f]}\right)\left(\frac{1}{(1 - f)R_{\mathrm{I}} + f}\right)$$

with strategies

$$e^*_{\mathrm{I}}(0) = 1$$

$$e^*_{\mathrm{I}}(1) = \frac{R_{\mathrm{II}} - f}{(1 - f)[(1 - 2f)R_{\mathrm{II}} + 2f]} - \left(\frac{1 - \delta}{\delta}\right)\frac{[1 - (1 - f)(1 - 2f)](1 - R_{\mathrm{II}})}{(1 - f)[(1 - 2f)R_{\mathrm{II}} + 2f]}$$

$$e^*_{\mathrm{I}}(n) = 0 \quad \forall n \geqslant 2$$

$$e^*_{\mathrm{II}}(1) = \left(\frac{1 - \varrho}{\varrho}\right)\left(\frac{\beta^*_{\mathrm{I}}(1)}{1 - \beta^*_{\mathrm{I}}(1)}\right)$$

$$e^*_{\mathrm{II}}(n) = 0 \quad \forall n \geqslant 2$$

$$e^*_{\mathrm{II'}}(1) = 1$$

$$e^*_{\mathrm{II'}}(2) = 1$$

$$e^*_{\mathrm{II'}}(n) = 0 \quad \forall n \geqslant 3$$

$$e^*_{\mathrm{I'}}(n) = 1 \quad \forall n \leqslant N_{\mathrm{I'}}$$

$$e^*_{\mathrm{I'}}(n) = 0 \quad \forall n > N_{\mathrm{I'}}$$

and with the relevant beliefs

$$\beta^*_{\mathrm{I}}(0) = \beta^*_{\mathrm{I'}}(0) = \varrho$$

$$\beta^*_{\mathrm{I}}(1) = \beta^*_{\mathrm{I'}}(1) = \frac{[1 - (1 - 2f)(1 - 3f)](1 - R_{\mathrm{I}})}{(1 - 2f)[(1 - 3f)R_{\mathrm{I}} + 3f]}$$

$$\beta^*_{\mathrm{I}}(2) = \beta^*_{\mathrm{I'}}(2) = 0$$

$$\beta^*_{\mathrm{II}}(1) = \beta^*_{\mathrm{II'}}(1) = \delta$$

$$\beta^*_{\mathrm{II}}(2) = \beta^*_{\mathrm{II'}}(2) = \frac{\delta e^*_{\mathrm{I}}(1)}{1 - \delta[1 - e^*_{\mathrm{I}}(1)]}$$

$$\beta^*_{\mathrm{II'}}(3) = \beta^*_{\mathrm{II'}}(3) = 0$$

Equilibrium 4 exists only if

$$1 - \left(\frac{R_{\mathrm{II}} - f}{(1 - f)[(1 - 2f)R_{\mathrm{II}} + 2f]}\right)\left(\frac{R_{\mathrm{II'}} - 3f}{(1 - 3f)[(1 - 4f)R_{\mathrm{II'}} + 4f]}\right) > \delta > \frac{[1 - (1 - 3f)(1 - 4f)](1 - R_{\mathrm{II'}})}{(1 - 3f)[(1 - 4f)R_{\mathrm{II'}} + 4f]}$$

$$\varrho > 1 - \left(\frac{w_{\mathrm{I}} - q_{\mathrm{I}}}{w_{\mathrm{I}} - d_{\mathrm{I}}}\right)\frac{1}{(1 - f)R_{\mathrm{I}} + f}$$

with strategies

$$e^*_{\mathrm{I}}(0) = 1$$

$$e^*_{\mathrm{I}}(1) = \left(\frac{1 - \delta}{\delta}\right)\frac{[1 - (1 - 3f)(1 - 4f)](1 - R_{\mathrm{II'}}}{R_{\mathrm{II'}} - 3f}$$

$$e^*_{\mathrm{I}}(n) = 0 \quad \forall n \geqslant 2$$

$$e^*_{\mathrm{II}}(n) = 0 \quad \forall n \geqslant 1$$

$$e^*_{\mathrm{II'}}(1) = \left(\frac{1}{1 - \varrho}\right)\left(\frac{w_{\mathrm{I}} - q_{\mathrm{I}}}{w_{\mathrm{I}} - d_{\mathrm{I}}}\right)\left(\frac{1}{(1 - f)R_{\mathrm{I}} + f}\right)$$

$$e^*_{\mathrm{II'}}(2) = \frac{R_{\mathrm{I}} - 2f}{(1 - 2f)[(1 - 3f)R_{\mathrm{I}} + 3f]}$$

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$$e^*_{II'}(n) = 0 \qquad \forall n \geq 2$$

$$e^*_{II'}(n) = 1 \qquad \forall n < N_{I'}$$

$$e^*_{I'}(n) = 0 \qquad \forall n > N_{I'}$$

and with the relevant beliefs

$$\beta^*_{I}(0) = \beta^*_{I'}(0) = \varrho$$

$$\beta^*_{I}(1) = \beta^*_{I'}(1) = 0$$

$$\beta^*_{I}(2) = \beta^*_{I'}(2) = 0$$

$$\beta^*_{II}(1) = \beta^*_{II'}(1) = \delta$$

$$\beta^*_{II}(2) = \beta^*_{II'}(2) = [1 - (1 - 3f)(1 - 4f)] \left(\frac{1 - R_{II'}}{(1 - 3f)[(1 - 4f)R_{II'} + 4f]} \right)$$

The four equilibria described in Proposition 1 and derived in the Appendix share the same general pattern of crisis bargaining. In each equilibrium, each state begins with an initial belief about the type of its adversary. As the crisis unfolds, each state revises its belief in light of the strategies of its potential adversary. Generally, the longer the crisis lasts, the more confident a state becomes that it is facing a resolute adversary. Assuming that the states avoid accidentally falling over the brink and suffering the disaster of an unlimited nuclear exchange, then eventually one of the states finds the risk of continuing too great given its current belief about the type of its adversary and its strategy. This state then submits.

Although the four equilibria share the same general pattern of bargaining, there are specific differences among them. These differences are due to the different initial beliefs, that is, to the different values of δ and ϱ. First, compare Equilibria 1, 2, and 3. In all three, δ, which is the probability of an irresolute potential challenger, must be relatively high, and it is possible to satisfy the constraint on δ in all three equilibria simultaneously. It is, however, impossible for any value of ϱ, which is the probability of the challenged state being irresolute, to satisfy more than one of these constraints. For small ϱ, Equilibrium 1 exists. For intermediate ϱ, Equilibrium 2 exists. For large ϱ, Equilibrium 3 exists.

The effects of variations in ϱ are most immediately evident in the irresolute potential challenger's decision of whether or not to exploit the situation. The larger ϱ, the more confident I is that it is facing an irresolute adversary and the more likely it is to challenge the status quo. That is, $e^*_{I}(0)$ is smallest in Equilibrium 1, larger in Equilibrium 2, and largest in Equilibrium 3.

If there is a challenge, then, because I is least likely to escalate in Equilibrium 1, II is the most confident in this equilibrium that the challenger is resolute. Indeed, II is so confident that it is facing I′ that it never escalates: $e^*_{II}(0) = 0$ for all n. Since II never escalates, a challenger in Equilibrium 1 is certain that it is facing a resolute adversary if its challenge is resisted. Thus, I is certain that it is facing II′ if I's challenge is resisted. But although certain that its adversary is resolute, I does not necessarily submit. Rather, I escalates with probability $e^*_{I}(1)$. The justification for this seemingly irrational act is that II′ is uncertain of the challenger's resolve. In fact, II′ is so confident that it is facing a resolute challenger, the chances that it will submit are sufficiently large that it is worth it to I to exploit II's uncertainty by escalating. So, I escalates although certain that its adversary's resolve is greater than its own.

In Equilibrium 2, II is more confident that it is confronting an irresolute challenger and now may resist a challenge by escalating with probability $e^*_{II}(1)$. I in turn is more confident that it is facing an irresolute adversary if its challenge is resisted. Nevertheless, I is more likely to meet resistance with escalation in Equilibrium 1 where it is certain that it is facing a resolute adversary than in Equilibrium 2 where it is more confident that it is con-

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fronting an irresolute adversary. This is explained by the fact that II′ is more likely to reply to a challenger's escalation by escalating a second time in Equilibrium 2. Thus a state may be more confident that it is facing an irresolute adversary yet be less likely to escalate.

In Equilibrium 3, ϱ is so high and I is so confident that it is facing an irresolute adversary that I, like I′, is certain to challenge. Because I and I′ follow the same strategy in deciding whether to exploit the situation, a challenged state learns nothing about the resolve of its adversary from the initial strategy of the challenger. A challenge reveals no information about the type of the challenger.

Because I is most likely to challenge in this equilibrium, its adversary is most confident that it is facing I. Indeed, II′ is so confident that it is facing an irresolute challenger that it is certain to escalate for all $n \leqslant N_{II'}$. Revealing nothing about its type does, however, bring a balancing advantage for I. With large ϱ, II is less likely to escalate in this equilibrium than in Equilibrium 2. That is, in Equilibrium 3 $e^*_{II}(1)$ is smaller than in Equilibrium 2 where I's strategy is not identical to that of I′ and I reveals some information about its type.

Finally, consider Equilibrium 4 by comparing it to Equilibrium 3. Both equilibria require ϱ to be large. But the probability of an irresolute potential challenger, δ, is smaller in Equilibrium 4 than it was in Equilibrium 3. Because δ is smaller in the former than the latter and I is certain to challenge in both equilibria, the challenged state is less confident of facing an irresolute challenger in the former. In fact, II′ was so confident that its adversary in Equilibrium 3 was I that it was certain to escalate for all $n \leqslant N_{II'}$. Being less confident of facing I in Equilibrium 4, II′ is throughout the crisis less likely to escalate than in Equilibrium 3 where II′ was certain to escalate for all $n \leqslant N_{II'}$.

Resolve, Misperception, the Status Quo and the Dynamics of Escalation

I now examine the effects of changes in the level of resolve, the degree of misperception, and the value of the status quo on crisis stability and the dynamics of escalation. The analysis is complicated by the existence of multiple equilibria that tend to mirror each other. For example, an increase in the resolve of I due to a higher return to prevailing, that is, to an increase in w_I, makes II more likely to escalate at $\theta_{II}(1)$ in Equilibrium 2 but less likely to escalate in Equilibrium 3. Nevertheless, the four equilibria do support some generalizations. The characterization of a brinkmanship crisis as a contest of resolve is misleading. Moreover, reducing the level of misperception may reduce, not increase, crisis stability. Finally, a small increase in the irresolute potential challenger's stake in the status quo does not make a challenge to the status quo less likely.

Before taking up this examination, a preliminary observation is in order. Although each state is uncertain of its adversary's resolve and this is modeled by letting Nature begin the game by randomly selecting the levels of resolve, the phenomenon being modeled is a crisis in which each state's resolve, while unknown to its adversary, is fixed. Viewed in this way, Proposition 1 describes the escalatory dynamics of four different types of crisis. These types are described by the types of state that are actually facing each other in the crisis. In, for example, a crisis in which both states are actually irresolute, escalation follows the dynamic specified by the strategies of I and II in Proposition 1. If, however, the actual crisis involved two resolute states, then the interaction of the strategies of I′ and II′ would determine the pattern of

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escalation. Let (I,II), (I,II′), (I′,II) and (I′,II′) denote these crises.

Although there are four distinct types of crisis and each has its own escalatory dynamic, it is important to realize that these types of crisis cannot be separated completely. They are linked by the states' beliefs. For example, an actual crisis of type (I,II) is linked to (I,II′) because the actual challenger, I, is uncertain of the type of its adversary. I does not know if it is facing II or II′. That is, I does not know if it is in (I,II) or (I,II′). Similarly, the beliefs of II link the actual crisis (I,II) to (I′,II). This linkage means that a crisis cannot be described solely in terms of the resolve or, more generally, the types of state in the crisis. The states' beliefs about the resolve or the potential type of their adversary are an integral part of the description of a crisis.

Crisis Stability

The existence of different types of crisis naturally leads to questions about their relative stability. Intuition suggests that a crisis in which both states are resolute should be more likely to end in war and hence be less stable than a crisis in which only one state is resolute. Furthermore, a crisis in which one state is resolute should be less stable than a crisis in which neither state is resolute. As will be shown, the model supports this intuition.

Crisis stability is usually taken to be a measure of the likelihood of a crisis ending in war. The more stable the crisis, the less likely war. This idea is easily formalized in the model. For example, the probability of, say, a crisis of type (I,II′) ending in war is $P_W((I,II')) = f + (1 - f)e^*_I(1)\,[2f + (1 - 2f)e^*_{II'}(2)3f]$. The probability of a crisis not ending in disaster is $1 - P_W$, and this will be taken as the measure of crisis stability.

P_W is calculated from the point at which Nature first has a chance to play disaster. At this point, there is a crisis: there has been a resisted challenge. Calculating P_W in this way raises an issue that will become important later but should be introduced here because it affects the way that P_W is calculated. The probability of a situation of type (I,II′) ending in disaster would not be calculated from where Nature first has a chance to impose disaster. Rather, this probability would be calculated from the point at which a potential challenger is deciding if it will exploit the situation. For I, this probability would be calculated from $\theta_I(0)$ and would equal $e^*_I(0)e^*_{II'}(1)P_W$. Letting S_W denote this probability, then $1 - S_W$ measures "situational" stability, which is the probability of a situation not escalating to disaster. In general, situational stability will not equal crisis stability and, more importantly, changes in the model's parameters that, for example, increase situational stability may not increase crisis stability. Thus, the distinction between crisis stability and situational stability or, more generally, the distinction between statements made about a historical sample that only includes crises and statements about a historical sample that includes situations that could have become crises but did not is sometimes crucial. Indeed, this distinction will play an important role in the discussion of the effects of misperception on stability. However, the following conclusions about the relative stability of the four types of crisis hold for both crisis and situational stability.

The crises may be ranked by noting that a resolute state is always at least as likely to escalate and sometimes more likely to escalate than its irresolute counterpart. Thus, $P_W((I',II')) > P_W((I',II)) > P_W((I,II))$ and $P_W((I',II')) > P_W((I,II')) > P_W((I,II))$. Then, a severe crisis that entails a grave conflict of interest because both sides are resolute is more dangerous and less stable than a crisis in which only one state is resolute. A minor crisis in which neither state is resolute is less dangerous and more stable than one in which one or

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both states are resolute. The model agrees with the intuitive ranking of these crises by their relative stability.

Resolve

Several of the conclusions about the effects of changes in the states' levels of resolve suggested by models based on critical risks do not hold up when nuclear brinkmanship is explicitly modeled as a well-defined game of sequential bargaining with one-sided incomplete information (Powell 1987). Jervis argues that in a contest of resolve, the state "willing to run the greatest risks will prevail" (1979b, 631). Moreover, critical-risk models suggest that the greater a state perceives its adversary's resolve to be, the less likely the state is to escalate (Ellsberg 1959; Jervis 1972, 1978; Snyder 1971, 1972; and Snyder and Diesing 1977). Neither of these conclusions holds with one-sided incomplete information. Nor do they hold with two-sided incomplete information. Indeed, recall that I in Equilibrium 1 is certain that it is facing a more resolute adversary if its challenge is resisted. Nevertheless, I exploits its adversary's uncertainty about the resolve of the challenger by escalating with probability $e^*_{\mathrm{I}}(1)$. I, certain that its adversary's resolve is greater, is playing a strategy of pure bluff. But in this crisis, I will prevail with probability $e^*_{\mathrm{I}}(1)(1 - f)[1 - e^*_{\mathrm{II}'}(1)]$. Sometimes bluffing works and the state with the least resolve prevails. To see that an increase in an adversary's resolve may make a state more, not less, likely to escalate, note that in Equilibria 2 and 3 $\partial e^*_{\mathrm{I}}(1)/\partial R_{\mathrm{II}} > 0$. In a (I,II)-crisis in these equilibria, an increase in the resolve of I's adversary makes I more likely to escalate.

The richer bargaining dynamic permitted by two-sided incomplete information also reveals something new about the effects of changes in the level of resolve. Note that in Equilibrium 3, $\partial e^*_{\mathrm{I}}(0)/\partial R_{\mathrm{II}} < 0$ but $\partial e^*_{\mathrm{I}}(1)/\partial R_{\mathrm{II}} > 0$. Hence, the effects of changes in an adversary's resolve may differ depending on the stage of the crisis. An increase in II's resolve in a (I,II)-crisis in this equilibrium makes I initially less likely to exploit the situation. But if I overcomes its greater initial reluctance to dispute the status quo, I pursues its challenge more tenaciously in that it is more likely to escalate if its reluctantly made challenge is resisted.

The remaining discussion of the effects of changes in the level of resolve shows that describing a brinkmanship crisis as a contest of resolve is misleading. Many propositions that intuitively seem to follow from this description turn out to be incorrect when examined formally. One of these propositions has already been noted. Although the notion of a contest of resolve suggests that the state with the greatest resolve will prevail, this is incorrect.

Another proposition that might easily be inferred from this description is that a state would be more likely to prevail, the greater its resolve. But consider a (I,II)-crisis in Equilibria 2 and 3. The probability of II prevailing is $(1 - f)[1 - e^*_{\mathrm{I}}(1)]$. This probability decreases as R_{II} increases. Thus, II is less likely to prevail the greater its resolve.

Still another proposition that might loosely be derived from the description of brinkmanship as a contest of resolve is that the greater a state's resolve, the greater its expected payoff should be. Being more resolute may not help in any given instance, but it should help on average. This proposition is also incorrect. Consider I's expected payoff in a (I,II')-crisis in Equilibrium 3. This is given by

$$fd_{\mathrm{I}} + (1 - f)([1 - e^*_{\mathrm{I}}(1)]s_{\mathrm{I}} + e^*_{\mathrm{I}}(1)\{2fd_{\mathrm{I}} + (1 - 2f)[3fd_{\mathrm{I}} + (1 - 3f)s_{\mathrm{I}}]\}).$$

Recalling that $R_{\mathrm{I}} = (w_{\mathrm{I}} - s_{\mathrm{I}})/(w_{\mathrm{I}} - d_{\mathrm{I}})$,

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suppose that the resolve of I is greater because s_I is smaller. Since I's expected payoff decreases as s_I decreases, its expected payoff decreases as its resolve increases.

Perhaps the least demanding proposition suggested by the notion of a contest of resolve is that the states' strategies should be functions of their levels of resolve. That is, the states' levels of resolve summarize enough information about the payoff structure of the situation to determine their strategies.[6] But even this is not the case. In Equilibrium 2, for example, $e^*_{II}(1)$ and $e^*_{II'}(2)$ depend on a combination of the payoffs of I that cannot be expressed as a function of the states' levels of resolve. In the contest of resolve, resolve does not even fully describe the dynamics of the contest.

In sum, many propositions that at first seem to follow from the description of a contest of resolve do not follow after a closer examination. Describing a brinkmanship crisis as a contest of resolve may obscure more than it clarifies.

Misperception and Crisis Stability

In the model, the situation actually facing a state can be distinguished from the situation that the state believes itself to be facing. Indeed, it is possible to specify the degree to which a state misperceives its situation. Suppose, for example, that I and II' are actually facing each other. At $\theta_I(0)$, I believes that it is facing II with probability ϱ. Since I is actually facing II', the strength of its belief that it is facing II, that is, the value of ϱ, measures the degree of I's misperception about the type of its adversary. Conversely, had I actually been facing II, then $1 - \varrho$ would measure the degree of misperception. Given the types of adversaries actually facing each other, the probabilities δ and ϱ measure their initial misperceptions. As the crisis escalates, each state revises its beliefs about its adversary in light of its actions and this affects the degree of misperception.

An important qualification to this formulation of misperception must be noted before the effects of misperception are examined. The probabilities δ and ϱ are assumed to be common knowledge. Thus each state knows how badly its adversary misperceives it. For example, I, knowing its type, knows that its adversary's degree of misperception is $1 - \delta$, and it knows that its adversary knows this. This assumed knowledge of the other's degree of misperception may be a very serious limitation of this formulation.

If misperception could be completely eliminated, each state would know its adversary's resolve. There would be complete information and no crises (Powell 1987). But what if misperception is reduced but not eliminated? Table 1 summarizes the effects of small reductions in the degree of misperception on crisis and situational stability. (Remember that crisis stability is a measure of the probability of the game ending in disaster once there has been a resisted challenge, whereas situational stability is the probability of this outcome once Nature has selected the states' types but before the potential challenger decides whether or not to dispute the status quo.) These effects are derived by evaluating the sign of the partial derivatives of the probability of war with respect to δ and ϱ. Suppose, for example, I and II are facing each other. P_W is the probability of a crisis of this type ending in war, and $P_W = f + (1 - f)e^*_I(1)2f$. The probability of the situation ending in war, S_W, is $e^*_I(0)e^*_{II}(1)P_W$. Now, $\partial S_W/\partial \varrho > 0$ in Equilibrium 2. That is, the situation becomes less stable as ϱ increases. I, moreover, believes that it is facing II with probability ϱ. As ϱ increases, misperception decreases. The situation thus becomes less stable as misperception decreases, and a "—" appears in the cell in the top row under Equilibrium 2 and *Situ-*

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Table 1. The Effects of Reducing Misperception on Crisis and Situational Stability

Type of Crisis	Type of Equilibrium 1 Situation	1 Crisis	2 Situation	2 Crisis	3 Situation	3 Crisis	4 Situation	4 Crisis
I	0	0	−	0	+	0	0	0
II	0	0	+	0	−	−	0	0
I	+	0	−	−	0	0	+	0
II′	+	0	+	0	−	−	−	−
I′	0	0	−	0	+	0	0	0
II	0	0	0	0	0	0	0	0
I′	+	0	+	+	0	0	+	0
II′	0	0	0	0	0	0	0	0

ation. The other cells are evaluated in the same way: "+" denotes an increase in stability due to a decrease in misperception; "−" denotes a decrease in stability; and *0* denotes no effect.

Table 1 shows that except for I′, reducing a state's misperception can be destabilizing as well as stabilizing. The effects of more accurate perceptions depend very much on the situation that is perceived more clearly.

An overdrawn example demonstrates how reducing misperceptions can reduce stability by making war more likely. Suppose that a potential challenger is actually facing an irresolute adversary. But this state also misperceives the resolve of its adversary and is very confident that its adversary is resolute. Given this misperception, the state will not challenge or resist its adversary. There will be no crisis and no chance of war. If, however, the state more accurately perceives its adversary's low resolve, the state may challenge or resist its adversary. This creates a crisis and some chance of war. More accurate perceptions in this case have reduced stability.

The Status Quo

Detente is in part based on the belief that the greater the stake a potential challenger has in the status quo, the less likely this state is to put this more valuable stake at risk by challenging the status quo. Although this is an assertion about foreign policy and not about crisis bargaining, it has an obvious parallel in crisis bargaining. In the model, this parallel does not hold. An increase in the value of the status quo to the potential challenger has a more complicated effect on the escalatory dynamics. An increase in the challenger's stake in the status quo does make a crisis less likely. But at least for small increases in the value of the status quo, a potential challenger is not less likely to make a challenge. Rather, the challenged state is less likely to resist by escalating.

To see that an increase in the value of the status quo makes a crisis less likely, consider first the effect of a large increase in the value of the status quo in Equilibria 2 and 4. For these equilibria to exist $\varrho > 1 - (w_I - q_I)/\{(w_I - d_I)[(1 - f)R_I + f]\}$. For any value of ϱ less than one that satis-

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fies this relation, there exists a new, larger value of q_I for which this relation does not hold and for which these equilibria do not exist. A similar argument shows that if q_I is large enough, Equilibrium 3 does not exist.

But now consider the effects of a smaller increase in q_I on the escalatory dynamics of any one of the equilibria. The strategies $e^*_I(0)$ and $e^*_I(1)$ do not depend on q_I. Changes in q_I do not affect I's strategy. An increase in the value of the status quo to I does not make I less likely to challenge the status quo. The strategies that may be affected by changes in q_I are $e^*_{II}(1)$, $e^*_{II'}(1)$, and $e^*_{II'}(2)$. When they are, an increase in q_I makes II and II' less likely to escalate if challenged. Crises thus becomes less likely but only because resistance is less likely.

The intuition behind this counterintuitive result is that an increase in the value of a potential challenger's stake in the status quo affects the bargaining dynamics of the crisis through two opposing influences. The first is the belief noted above that the more valuable the stake in the status quo, the more a challenger must risk by disputing the status quo. This influence would seem to make a challenge less likely.

The second, opposing influence is more subtle. The heart of a brinkmanship crisis is each state's attempt to convince its adversary that it is resolute. This is what drives escalation. But now suppose that a state, uncertain of a potential challenger's resolve, knows that the potential challenger's stake in the status quo is large. Then there is a challenge. What is the challenged state to infer from a challenge that put a more valuable stake at risk? A reasonable inference would be that the challenger is more determined to have its way than it would have seemed if it had only had to risk a less valuable stake. That is, by risking a more valuable stake, a challenger signals greater determination and greater resolve. Concluding that the challenger's resolve is greater, the challenged state is less likely to escalate and more likely to submit. In essence, a potential challenger can use its adversary's knowledge that the status quo is more valuable to the challenger as a bargaining lever. With this lever, a challenger can convey an impression of greater resolve to its adversary by disputing the more valuable status quo.

These two influences oppose each other. In the model, the latter dominates for relatively small changes in the value of the status quo. But the precise balance in the model is less important than that the model has exposed an influence that, once seen, seems intuitively plausible and yet has not been appreciated previously.

The Distribution of Crises

Jervis has argued that one consequence of the nuclear revolution is that crises "should be in peripheral areas where neither side's stake is very high" (1986, 695). The model provides some weak support for the argument that the states' stakes in a brinkmanship crisis will be small. In the model, the probability of the challengers in a crisis being resolute is less than the probability of the challengers being resolute. Moreover, a crisis in which both states are resolute is less likely than crises in which at least one state is irresolute.

Showing this requires a reinterpretation of the meaning of δ and ϱ. Until now a crisis has been assumed to be of a given type and δ and ϱ measured the states' uncertainty about the actual type of crisis. Suppose instead that δ and ϱ are measures of the distribution of interests in the international system so that the probability of a situation of type (I,II) is $\delta\varrho$, of (I,II') is $\delta(1 - \varrho)$, of (I',II) is $(1 - \delta)\varrho$, and of (I',II') is $(1 - \delta)(1 - \varrho)$.

The demonstration that the probability of a challenger being irresolute is greater

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than the probability of it being resolute, given a crisis, begins by letting M denote the probability of a crisis. Then $M = \delta e^*_{\mathrm{I}}(0)[\varrho e^*_{\mathrm{II}}(1) + (1 - \varrho)e^*_{\mathrm{II'}}(1)] + (1 - \delta)[\varrho e^*_{\mathrm{II}}(1) + (1 - \varrho)e^*_{\mathrm{II'}}(1)]$. The probability of an irresolute challenger, given a crisis, is $\delta e^*_{\mathrm{I}}(0)[\varrho e^*_{\mathrm{II}}(1) + (1 - \varrho)e^*_{\mathrm{II'}}(1)]/M$ and of a resolute challenger is $(1 - \delta)[\varrho e^*_{\mathrm{II}}(1) + (1 - \varrho)e^*_{\mathrm{II'}}(1)]/M$. To demonstrate that the former is larger than the latter, it will suffice to show $\delta e^*_{\mathrm{I}}(0) > (1 - \delta)$. To do this for Equilibrium 1, rewrite the expression for $e^*_{\mathrm{I}}(0)$ in Proposition 1 to obtain an expression for $\delta e^*_{\mathrm{I}}(0)/(1 - \delta)$. If this new expression can be shown to be greater than 1, then $\delta e^*_{\mathrm{I}}(0)/(1 - \delta) > 1$ and the demonstration will be complete.

The greatest lower bound for the expression for $\delta e^*_{\mathrm{I}}(0)/(1 - \delta)$ is obtained by evaluating this expression at the least upper bound of $R_{\mathrm{II'}}$ which is $5f$. Substituting this value for $R_{\mathrm{II'}}$ in the inequality $\delta e^*_{\mathrm{I}}(0)/(1 - \delta) > 1$ gives a relation in f, which will be satisfied if f, which is already assumed to be less than .25, is further restricted to being less than .1. This restriction on f merely means that the least resolute state, II, is not willing to hazard a chance of disaster of more than 30%. Thus, if $f < .1$, a challenger is more likely to be irresolute in Equilibrium 1. Similar arguments show that if $f < .1$, then a challenger in a crisis in the other equilibria is more likely to be irresolute.

The fact that the challenger is more likely to be irresolute immediately leads to the conclusion that given a crisis, the probability of it being severe is less than 1/2. This probability will be less than 1/2 if and only if $(1 - \delta)(1 - \varrho)e^*_{\mathrm{II'}}(1) < M - (1 - \delta)(1 - \varrho)e^*_{\mathrm{II'}}(1)$. But

$$\begin{aligned} M - (1 - \delta)(1 - \varrho)e^*_{\mathrm{II'}}(1) &= \delta\varrho e^*_{\mathrm{I}}(0)e^*_{\mathrm{II}}(1) \\ &\quad + \delta(1 - \varrho)e^*_{\mathrm{I}}(0)e^*_{\mathrm{II'}}(1) + (1 - \delta)\varrho e^*_{\mathrm{II'}}(1) \\ &> (1 - \delta)\varrho e^*_{\mathrm{II}}(1) + (1 - \delta)(1 - \varrho)e^*_{\mathrm{II'}}(1) \\ &\quad + (1 - \delta)\varrho e^*_{\mathrm{II'}}(1) \\ &> (1 - \delta)(1 - \varrho)e^*_{\mathrm{II'}}(1) \end{aligned}$$

where the first inequality follows because $\delta e^*_{\mathrm{I}}(0) > 1 - \delta$. Thus, given a crisis, the probability of it entailing a severe conflict of interest is less than the probability of it not entailing a severe conflict.

The fact that a challenger is less likely to be resolute and that a crisis is less likely to be severe can be explained as a type of sampling bias due to concentrating on crises and not on situations. In the model, the distribution over the four types of situations is completely arbitrary because the probabilities δ and ϱ are arbitrary. For example, the international system may be characterized by many severely conflictual situations. That is, the probability of a (I′,II′)-situation, $(1 - \delta)(1 - \varrho)$, may be large. But this does not imply that severely conflictual crises are most likely. Although the distribution of situations is arbitrary, the distribution of crises is not arbitrary because it reflects the states' acting on their beliefs about the situation. With $1 - \delta$ large, a challenged state is very confident that the challenger is resolute and is unlikely to resist the challenge. But if there is no resistance, there is no crisis and this situation is never included in a sample consisting only of crises. The effect of this sampling bias is that crises are less likely to entail a severe conflict of interest.

Conclusion

I have extended the analysis of nuclear brinkmanship to the more natural setting in which there is two-sided incomplete information. The game's sequential crisis equilibria support some claims about crisis bargaining and escalation and raise doubts about others. Intuitively, crises involving a grave conflict of interest would seem to be the least stable and the most dangerous, whereas crises in which there is only a minor conflict of interest would seem to be the most stable and least dangerous. The model supports this claim.

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But the model contradicts the claims that the state with the greatest resolve prevails or that the greater an adversary's resolve, the less likely a state is to escalate. Indeed, for an irresolute potential challenger, an increase in its adversary's resolve may make the potential challenger less likely to dispute the status quo. But if it does challenge the status quo, it is more likely to meet resistance with escalation.

The analysis also shows that describing a brinkmanship crisis as a contest of resolve is misleading. Contrary to what might be presumed to follow from this description, a state may be less, not more, likely to prevail, the greater its resolve. A state's expected payoff may be less, not more, the greater its resolve. In fact, it is not even possible to express the state's strategies as functions of the states' levels of resolve. Describing a brinkmanship crisis as a contest of resolve may obscure as much as it clarifies.

Because the situation actually facing a state may be distinguished from the situation a state believes itself to be facing, certain aspects of the role of misperception in crisis bargaining may be formalized. Although there would be no crises if misperception could be completely eliminated, reducing, but not eliminating, misperception can be stabilizing or destabilizing.

Increasing a potential challenger's stake in the status quo would seem to reduce the likelihood of this state disputing the status quo, for to do so would be to risk a more valuable stake. The model showed this assertion to be problematic. A greater stake in the status quo may be used by the challenger as a bargaining lever against its adversary to convey an impression of greater resolve to the adversary. The result is that a potential challenger is not less likely to challenge the status quo. But a challenged state is more likely to submit.

Some weak statements about the distribution of crises may also be made. Given a crisis, a challenger is, surprisingly, more likely to be irresolute than resolute. A severe crisis is, moreover, less likely than a crisis that is not severe.

In an examination of the bargaining that takes place between a buyer and seller, Fudenberg and Tirole "found that some very intuitive results cannot be taken for granted. . . . The moral is that general assertions about the effects of parameter changes on the bargaining process are suspect" (1983, 239). The same would seem to apply to crisis bargaining.

Appendix

Proposition 1 is proved through backwards programming. As noted above, no state can attach a positive probability to attacking at any information set. Accordingly, determining the sequential equilibria means specifying $e^*_{\mathrm{I}}(m)$ and $e^*_{\mathrm{I'}}(m)$ for $0 \leqslant m \leqslant (K/2)$, $e^*_{\mathrm{II}}(n)$ and $e^*_{\mathrm{II'}}(n)$ for $1 \leqslant n \leqslant (K/2)$, and a consistent set of beliefs.

From the definition of the upper bound N_{I}, I's best reply at $\theta_{\mathrm{I}}(n)$ for $n > N_{\mathrm{I}} = 1$ is to submit: $e^*_{\mathrm{I}}(n) = 0$. Similarly, $e^*_{\mathrm{II}}(n) = 0$ for $n > N_{\mathrm{II}} = 1$; $e^*_{\mathrm{II'}}(n) = 0$ for $n > N_{\mathrm{II'}} = 2$; and $e^*_{\mathrm{I'}}(n) = 0$ for $n > N_{\mathrm{I'}} \geqslant 2$.

Now, $e^*_{\mathrm{I'}}(n) = 1$ for $N_{\mathrm{I'}} \geqslant n \geqslant 2$. If I′ escalates at $\theta_{\mathrm{I'}}(n)$ for $N_{\mathrm{I'}} \geqslant n \geqslant 2$, its adversary, regardless of type, will not subsequently escalate. The risk is too high. But then from the definition of $N_{\mathrm{I'}}$, the payoff to I′ of escalating at these information sets is greater than the return to submitting. So, $e^*_{\mathrm{I'}}(n) = 1$ for $N_{\mathrm{I'}} \geqslant n \geqslant 2$.

Recall that the game was simplified above by assuming that I′ would escalate at $\theta_{\mathrm{I'}}(0)$ and $\theta_{\mathrm{I'}}(1)$ regardless of what its adversary does. This, it was observed, may further restrict $N_{\mathrm{I'}}$ beyond the existing constraint that $N_{\mathrm{I'}} \geqslant 2$. The worst that can happen to I′ is that it is facing a resolute adversary that escalates with prob-

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ability 1 at $\theta_{II'}(1)$ and $\theta_{II'}(2)$. Since $N_{II'} = 2$, the resolute adversary II′ will then submit. To ensure that I′ escalates at $\theta_{I'}(0)$ and $\theta_{I'}(1)$ in this worst case, it will suffice to assume $R_{I'} > 1 - (1 - f)(1 - 2f)(1 - 3f)(1 - 4f) + (q_{I'} - s_{I'})/(w_{I'} - d_{I'})$. For any f, this can always be done with a suitable choice of $w_{I'}$, $q_{I'}$, $s_{I'}$, and $d_{I'}$. If, moreover, $f < .1$, as is assumed in the discussion of the distribution of crises, then when $R_{I'}$ satisfies this inequality, $R_{I'}$ is also greater than $4f$ which ensures that $N_{I'} \geqslant 2$. Thus, taking $R_{I'}$ to satisfy this inequality and taking $f < .1$ guarantees $e^*_{I'}(0) = e^*_{I'}(1) = 1$.

The only strategies left to be specified are those at $\theta_I(0)$, $\theta_I(1)$, $\theta_I(1)$, $\theta_{II'}(1)$, and $\theta_{II'}(2)$. To find these, begin by considering the problem facing II′ if the crisis reaches $\theta_{II'}(2)$. If II′ is facing I, escalation brings $3fd_{II'} + (1 - 3f)w_{II'}$. If, however, II′ is facing I′, escalation yields $3fd_{II'} + (1 - 3f)[4fd_{II'} + (1 - 4f)s_{II'}]$. But II′ is uncertain whether it is facing I or I′, and the expected payoff to escalation is $\beta_{II'}(2)[3fd_{II'} + (1 - 3f)S_{II'}] + [1 - \beta_{II'}(2)]\{3df_{II'} + (1 - 3f)[4fd_{II'} + (1 - 4f)s_{II'}]\}$.

Let $B_{II'}(2)$ be the value of $\beta_{II'}(2)$ that leaves II′ indifferent between submitting and escalating at $\theta_{II'}(2)$. Then, $B_{II'}(2) = [1 - (1 - 3f)(1 - 4f)](1 - R_{II'})/\{(1 - 3f)[(1 - 4f)R_{II'} + 4f]\}$. So, the best response of II′ at $\theta_{II'}(2)$, is $e^*_{II'}(2) = 1$ if $\beta_{II'}(2) > B_{II'}(2)$; $e^*_{II'}(2) \in [0,1]$ if $\beta_{II'}(2) = B_{II'}(2)$; and $e^*_{II'}(2) = 0$ if $\beta_{II'}(2) < B_{II'}(2)$.

Beliefs are given by Bayes' rule where this rule can be applied. For $\theta_{II'}(2)$, this means $\beta_{II'}(2) = \delta e_I(0)e_I(1)/[\delta e_I(0)e_I(1) + (1 - \delta)e_{I'}(0)e_{I'}(1)]$. Recalling that $e^*_{I'}(0) = e^*_{I'}(1) = 1$ in equilibrium, substituting for $\beta_{II'}(2)$ shows that II′'s best reply is

$$e^*_{II'}(2) = 1 \quad \text{if} \quad e_I(1)e_I(0) > \left(\frac{1 - \delta}{\delta}\right)\left(\frac{B_{II'}(2)}{1 - B_{II'}(2)}\right)$$

$$e^*_{II'}(2) \in [0, 1] \quad \text{if} \quad e_I(1)e_I(0) = \left(\frac{1 - \delta}{\delta}\right)\left(\frac{B_{II'}(2)}{1 - B_{II'}(2)}\right)$$

$$e^*_{II'}(2) = 0 \quad \text{if} \quad e_I(1)e_I(0) < \left(\frac{1 - \delta}{\delta}\right)\left(\frac{B_{II'}(2)}{1 - B_{II'}(2)}\right)$$

Now observe that in no sequential crisis equilibrium can $e^*_{II'}(2) = 0$. To see this, assume that $e^*_{II'}(2) = 0$. Since II′ is certain to submit at $\theta_{II'}(2)$, I's best reply at $\theta_I(1)$ is to escalate: $e_I(1) = 1$. But if $e_I(1) = e_{I'}(1) = 1$, then $e_{II'}(1) = 0$, since it makes no sense for II′ to escalate at $\theta_{II'}(1)$ if it is certain to quit at $\theta_{II'}(2)$ and I and I′ are certain to escalate at $\theta_I(1)$ and $\theta_{I'}(1)$. Similarly, $e_{II}(1) = 0$. But this is a contradiction: if $e_{II}(1) = e_{II'}(1) = 0$, no challenge is resisted, and there is no crisis.

This observation implies that $e_I(1)e_I(0) \geqslant [(1 - \delta)/\delta]\{B_{II'}(2)/[1 - B_{II'}(2)]\}$ in any these equilibria requires the consideration of two cases. Equality is assumed to hold in the first and yields Equilibria 1, 2, and 4. Inequality is presumed in the second and this gives Equilibrium 3.

Case 1

Case 1 is defined by:

$$e_I(1)e_I(0) = [(1 - \delta)/\delta]\{B_{II'}(2)/[1 - B_{II'}(2)]\}$$

Consider the problem facing I at $\theta_I(1)$. The payoff to escalation is

$$\beta_I(1)[2fd_I + (1 - 2f)w_I] + [1 - \beta_I(1)] \cdot (2fd_I + (1 - 2f)\{[1 - e_{II'}(2)]w_I + e_{II'}(2)[3fd_I + (1 - 3f)s_I]\})$$

Let $e_{II'}(2)$ be the strategy of II′ at $\theta_{II'}(2)$ that leaves I indifferent to submitting and escalating. Then, $\hat{e}_{II'}(2) = (R_I - 2f)/\{[1 - \beta_I(1)](1 - 2f)[(1 - 3f)R_I + 3f]\}$. I's best response at $\theta_I(1)$ is $e_I = 0$ if $e_{II'}(2) > \hat{e}_{II'}(2)$; $e_I(1) \in [0, 1]$ if $e_{II'}(2) = \hat{e}_{II'}(2)$; and $e_I(2) = 0$ if $e_{II'}(2) < \hat{e}_{II'}(2)$.

The expression for $\hat{e}_{II'}(2)$ may be simplified by substituting for $\beta_I(1)$. By Bayes' rule, $\beta_I(1) = \varrho e_{II}(1)/[\varrho e_{II}(1) + (1 - \varrho)e_{II'}(1)]$. This gives

$$\hat{e}_{II'}(2) = \left[1 + \left(\frac{\varrho}{1 - \varrho}\right)\frac{e_{II}(1)}{e_{II'}(1)}\right]$$

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$$\left(\frac{R_{\mathrm{I}} - 2f}{(1-2f)[(1-3f)R_{\mathrm{I}} + 3f]}\right)$$

Now turn to the problem facing II at $\theta_{\mathrm{II}}(1)$. Let $\hat{e}_{\mathrm{I}}(1)$ be the value of $e_{\mathrm{I}}(1)$ that makes II indifferent to escalating and submitting. Then $\hat{e}_{\mathrm{I}}(1)$ satisfies

$$\begin{aligned} s_{\mathrm{II}} = {} & \beta_{\mathrm{II}}(1)(fd_{\mathrm{II}} + (1-f)\{[1-\hat{e}_{\mathrm{I}}(1)]w_{\mathrm{II}} \\ & + \hat{e}_{\mathrm{I}}(1)[2f + (1-2f)s_{\mathrm{II}}]\}) \\ & + [1-\beta_{\mathrm{II}}(1)]\{fd_{\mathrm{II}} + (1-f)[2f + (1-2f)s_{\mathrm{II}}]\} \end{aligned}$$

and this is given by

$$\hat{e}_{\mathrm{I}}(1) = 1 - \frac{[1-(1-f)(1-2f)](1-R_{\mathrm{II}})}{\beta_{\mathrm{II}}(1)(1-f)[(1-2f)R_{\mathrm{II}} + 2f]}$$

Substituting for $\beta_{\mathrm{II}}(1)$ yields

$$\hat{e}_{\mathrm{I}}(1) = 1 - \left[1 + \left(\frac{1-\delta}{\delta}\right)\frac{1}{e_{\mathrm{I}}(0)}\right] \left(\frac{[1-(1-f)(1-2f)](1-R_{\mathrm{II}})}{(1-f)[(1-2f)R_{\mathrm{II}} + 2f]}\right)$$

II's best reply at $\theta_{\mathrm{II}}(1)$ is $e_{\mathrm{II}}(1) = 0$ if $e_{\mathrm{I}}(1) > \hat{e}_{\mathrm{I}}(1)$; $e_{\mathrm{II}}(1) \in [0, 1]$ if $e_{\mathrm{I}}(1) = \hat{e}_{\mathrm{I}}(1)$; and $e_{\mathrm{II}}(1) = 1$ if $e_{\mathrm{I}}(1) < e_{\mathrm{I}}(1)$.

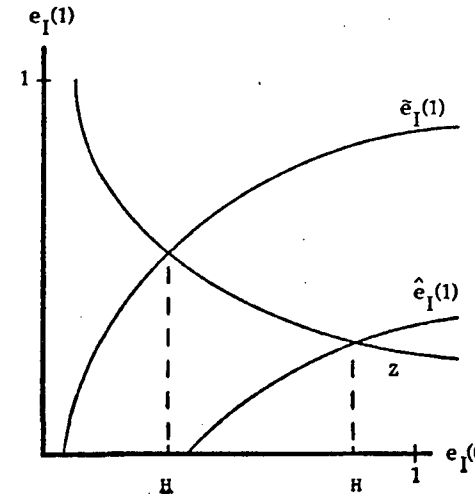
II′ faces a similar problem at $\theta_{\mathrm{II'}}(1)$. Let $\tilde{e}_{\mathrm{I}}(1)$ be the value of $e_{\mathrm{I}}(1)$ that leaves II′ indifferent to escalating and submitting. Then,

$$\tilde{e}_{\mathrm{I}}(1) = 1 - \left[1 + \left(\frac{1-\delta}{\delta}\right)\frac{1}{e_{\mathrm{I}}(0)}\right] \left(\frac{[1-(1-f)(1-2f)](1-R_{\mathrm{II'}})}{(1-f)[(1-2f)R_{\mathrm{II'}} + 2f]}\right)$$

The best response for II′ at $\theta_{\mathrm{II'}}(1)$ is $e_{\mathrm{II'}}(1) = 0$ if $e_{\mathrm{I}}(1) > \tilde{e}_{\mathrm{I}}(1)$; $e_{\mathrm{II'}}(1) \in [0,1]$ if $e_{\mathrm{I}}(1) = \tilde{e}_{\mathrm{I}}(1)$; and $e_{\mathrm{II'}}(1) = 1$ if $e_{\mathrm{I}}(1) < \tilde{e}_{\mathrm{I}}(1)$.

These best-reply correspondences will now be used to find points that are the best responses to themselves. Note that $\hat{e}_{\mathrm{I}}(1)$ and $\tilde{e}_{\mathrm{I}}(1)$ are increasing in $e_{\mathrm{I}}(0)$. Since $f < R_{\mathrm{II}} < 3f$ and $3f < R_{\mathrm{II'}} < 5f$, it is easy to show that for any $e_{\mathrm{I}}(0)$, $\hat{e}_{\mathrm{I}}(1) < \tilde{e}_{\mathrm{I}}(1)$. Figure 2 graphs $\hat{e}_{\mathrm{I}}(1)$ and $\tilde{e}_{\mathrm{I}}(1)$ and the constraint, Z, that defines Case 1. That is, Z is the curve given by $e_{\mathrm{I}}(1)e_{\mathrm{I}}(0) = [(1-\delta)/\delta]\{B_{\mathrm{II'}}(2)/[1 - B_{\mathrm{II'}}(2)]\}$. Because in Case 1 this constraint must be satisfied, only points along Z need be considered as potential equilibria.

Figure A-1. $\hat{e}_I(1)$ **and** $\tilde{e}_I(1)$



Let $\underline{H}$ be the value of $e_{\mathrm{I}}(0)$ at which $\tilde{e}_{\mathrm{I}}(1)$ satisfies the constraint and let H be the value of $e_{\mathrm{I}}(0)$ at which $\hat{e}_{\mathrm{I}}(1)$ satisfies the constraint. Because $\hat{e}_{\mathrm{I}}(1) < \tilde{e}_{\mathrm{I}}(1)$ for any $e_{\mathrm{I}}(0)$ and because $e_{\mathrm{I}}(1)$ is decreasing in $e_{\mathrm{I}}(0)$ along Z, $\underline{H} < H$.

$\underline{H}$ and H make it possible to specify the best replies of II and II′ at $\theta_{\mathrm{II}}(1)$ and $\theta_{\mathrm{II'}}(1)$. Considering only points along Z, then if, for example, $e_{\mathrm{I}}(0) < H$, it must be that $e_{\mathrm{I}}(1) > \hat{e}_{\mathrm{I}}(1)$. But this means that the best reply of II is $e_{\mathrm{II}}(1) = 0$. The best-reply correspondence of II at $\theta_{\mathrm{II}}(1)$ is given by $e_{\mathrm{II}}(1) = 0$ if $e_{\mathrm{I}}(0) < H$; $e_{\mathrm{II}}(1) \in [0,1]$ if $e_{\mathrm{I}}(0) = H$: and $e_{\mathrm{II}}(1) = 1$ if $e_{\mathrm{I}}(0) > H$. Similarly, the best-reply correspondence of II′ at $\theta_{\mathrm{II'}}(1)$ is given by $e_{\mathrm{II'}}(1) = 0$ if $e_{\mathrm{I}}(0) < \underline{H}$; $e_{\mathrm{II'}}(1) \in [0,1]$ if $e_{\mathrm{I}}(0) = \underline{H}$; and $e_{\mathrm{II'}}(1) = 1$ if $e_{\mathrm{I}}(0) > \underline{H}$.

In a sequential crisis equilibrium there must be some chance of a challenge being resisted. This means that $e_{\mathrm{I}}(0) \geqslant \underline{H}$, because if $e_{\mathrm{I}}(0) < \underline{H}$, then $e_{\mathrm{II}}(1) = 0$ and $e_{\mathrm{II'}}(1) = 0$. Thus the search for sequential equilibria may be limited to points along

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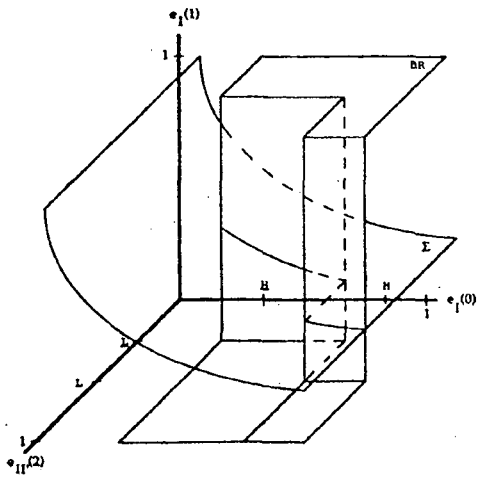
Figure A-2. The Best-Reply Correspondences



Z for which $e_I(0) \geqslant \underline{H}$. If, moreover, $e_I(0) = \underline{H}$, then $e_{II'}(1) > 0$.

Now consider I's best response if the game reaches $\theta_I(1)$. This depends on a comparison of $e_{II'}(2)$ and $\hat{e}_{II'}(2)$. This comparison may be transformed into one between $e_{II'}(2)$ and $e_I(0)$. Recall that $\hat{e}_{II}(2)$ is a function of $e_{II}(1)/e_{II'}(1)$. But in equilibrium, $e_{II}(1)$ and $e_{II'}(1)$ will be best replies to other strategies. These best replies are, as was just demonstrated, functions of $e_I(0)$. If, for example, $\underline{H} \leqslant e_I(0) < H$, the best replies of II and II' at $\theta_{II}(1)$ and $\theta_{II'}(1)$ are $e_{II}(1) = 0$ and $e_{II'}(1) > 0$. Hence, $\hat{e}_{II'}(2)$ evaluated at these best responses is $[R_I - 2f]/\{(1 - 2f)[(1 - 3f)R_I + 3f]\}$. Let $\underline{L}$ be this value and let L be the value of $\hat{e}_{II'}(2)$ evaluated at $e_{II}(1) = e_{II'}(1) = 1$. Then the transformed best-reply correspondence for I at $\theta_I(1)$ is

1	if	$(e_I(0), e_{II'}(2)) \in [\underline{H}, H) \times [0, \underline{L})$
[0,1]	if	$(e_I(0), e_{II'}(2)) \in [\underline{H}, H) \times \{\underline{L}\}$
0	if	$(e_I(0), e_{II'}(2)) \in [\underline{H}, H) \times (\underline{L}, 1]$
1	if	$(e_I(0), e_{II'}(2)) \in \{H\} \times [0, \underline{L})$
[0,1]	if	$(e_I(0), e_{II'}(2)) \in \{H\} \times [\underline{L}, L]$
0	if	$(e_I(0), e_{II'}(2)) \in \{H\} \times (L, 1]$
1	if	$(e_I(0), e_{II'}(2)) \in (H, 1] \times [0, L)$
[0,1	if	$(e_I(0), e_{II'}(2)) \in (H, 1] \times \{L\}$
0	if	$(e_I(0), e_{II'}(2)) \in (H, 1] \times (L, 1]$

This correspondence is illustrated in Figure 3 and will be called *BR*. Figure 3 also shows the constraining curve Z, which has now become a surface, Σ.

Remember that the problem is to specify the strategies at $\theta_I(0)$, $\theta_I(1)$, $\theta_{II}(1)$, $\theta_{II'}(1)$, and $\theta_{II'}(2)$. The first of these to fall out is $e_{II'}(2)$. If a sequential crisis equilibrium exists it must be in the intersection of *BR* and Σ. But at all points of intersection, $0 < e_I(1) < 1$. To see this, recall that Z approaches 0 asymptotically from above. So, $e_I(1) > 0$. Over $\underline{H} \leqslant e_I(0) \leqslant 1$, $e_I(1)$ attains its maximum on Σ at $e_I(0) = \underline{H}$. But from the definition of $\underline{H}$, $e_I(1) < 1$ at $e_I(0) = \underline{H}$. Since $0 < e_I(1) < 1$, the best-reply correspondence for I at $\theta_I(1)$ implies $e_{II'}(2) = \hat{e}_{II'}(2)$. In a sequential crisis equilibrium, $e^*_{II'}(2) = \hat{e}_{II'}(2)$. (Of course, $\hat{e}_{II'}(2)$ is still a function of $e_{II}(1)$ and $e_{II'}(1)$, so a complete determination of $\hat{e}_{II}(2)$ awaits their specification.)

At this point, it is necessary to consider two subcases. In the first, $e_I(0) \geqslant H$ and this will yield Equilibrium 2. The second subcase is defined by $\underline{H} \leqslant e_I(0) < H$, and this will give Equilibria 1 and 4.

The specification of Equilibria 2 begins by noting that because $e_I(0) \geqslant H > \underline{H}$ in the first subcase, then $e_{II'}(1) = 1$. Moreover, $1 \geqslant e_I(0) \geqslant H$. Accordingly, it is assumed that $H < 1$. The possibility that $H = 1$ is disregarded because it has measure zero. This is in keeping with the previous assumption that strict inequality held for resolve. That is, R_I, for example, was restricted to $f < R_I < 3f$ although the constraint that $N_I = 1$ only required $f \leqslant R_I < 3f$. This assumption was made to eliminate the multiple equilibria that arise only in the event that $R_I = f$ which has measure zero.

With $H < 1$, it follows by contradiction that $e_I(0) < 1$. Assume $e_I(0) = 1$. This implies that Σ and *BR* intersect at $e_I(0) = 1$. It has already been shown that $e_I(1) <$

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1. This means that as long as $H < 1$, the only place where $e_{\mathrm{I}}(0) = 1$ in this intersection is at $e_{\mathrm{II}'}(2) = L$. (When $H = 1$, $e_{\mathrm{II}'}(2)$ may be anything in the interval $[\underline{L}, L]$ in this intersection.) Since $e_{\mathrm{I}}(0) = 1 > H$, $e_{\mathrm{II}}(1) = 1$. Moreover, $e_{\mathrm{I}}(0) = 1$ implies that the payoff to I from exploiting the situation at $\theta_{\mathrm{I}}(0)$ is at least as great as the value of the status quo. This leaves $q_{\mathrm{I}} \leqslant \varrho[fd_{\mathrm{I}} + (1 - f)s_{\mathrm{I}}] + (1 - \varrho)[fd_{\mathrm{I}} + (1 - f)s_{\mathrm{I}}]$ where the expression for the return to exploiting the situation has been simplified by taking $e_{\mathrm{II}}(1) = e_{\mathrm{II}'}(1) = 1$ and noting that because $0 < e_{\mathrm{I}}(1) < 1$, I is randomizing at $\theta_{\mathrm{I}}(1)$ which means that the expected payoff if this information set is reached is s_{I}. The right side of this is less than s_{I}. This, however, is a contradiction since $q_{\mathrm{I}} > s_{\mathrm{I}}$. Hence, $H < 1$ means $0 < \underline{H} < e_{\mathrm{I}}(0) < 1$.

Since $0 < e_{\mathrm{I}}(0) < 1$, I must be indifferent to accepting the status quo and exploiting the situation. This leads directly to an expression for $e^*_{\mathrm{II}}(1)$. Indifference implies $q_{\mathrm{I}} = \varrho\{[1 - e^*_{\mathrm{II}}(1)]w_{\mathrm{I}} + e^*_{\mathrm{II}}(1)[fd_{\mathrm{I}} + (1 - f)s_{\mathrm{I}}]\} + (1 - \varrho)[fd_{\mathrm{I}} + (1 - f)s_{\mathrm{I}}]$. This gives

$$e^*_{\mathrm{II}}(1) = 1 - \frac{1}{\varrho}\left[1 - \left(\frac{w_{\mathrm{I}} - q_{\mathrm{I}}}{w_{\mathrm{I}} - d_{\mathrm{I}}}\right)\left(\frac{1}{(1 - f)R_{\mathrm{I}} + f}\right)\right]$$

Now that $e^*_{\mathrm{II}}(1)$ has been determined, $e^*_{\mathrm{II}'}(2)$ may be fully specified by evaluating $\hat{e}_{\mathrm{II}'}(2)$ at $e^*_{\mathrm{II}}(1)$ and $e^*_{\mathrm{II}'}(1)$. The result is the expression reported for Equilibrium 2 in Proposition 1.

The expression for $e^*_{\mathrm{II}}(1)$ places a restriction on the relation among the variables ϱ, q_{I}, R_{I}, and f. Examining it carefully yields the last two strategies that remain to be determined, $e^*_{\mathrm{I}}(0)$ and $e^*_{\mathrm{I}}(1)$. The expression for $e^*_{\mathrm{II}}(1)$ implies $e^*_{\mathrm{II}}(1) < 1$. The best-reply correspondence for II at $\theta_{\mathrm{II}}(1)$ shows that $e_{\mathrm{I}}(0) \leqslant H$ if $e^*_{\mathrm{II}}(1) < 1$. But $e_{\mathrm{I}}(0)$ has also been restricted to being larger than or equal to H. Thus, $e^*_{\mathrm{I}}(0) = H$. H, in turn, is the value of $e_{\mathrm{I}}(0)$ that satisfies the constraint $\hat{e}_{\mathrm{I}}(1) = [(1 - \delta)/\delta][B_{\mathrm{II}'}(2)/[1 - B_{\mathrm{II}'}(2)][1/e_{\mathrm{I}}(0)]$. Solving this for $e_{\mathrm{I}}(0)$ gives the expression for $e^*_{\mathrm{I}}(0)$ in Proposition 1. Substituting $e^*_{\mathrm{I}}(0)$ for $e_{\mathrm{I}}(0)$ in the constraint defining Case 1 yields the expression for $e^*_{\mathrm{I}}(1)$.

The expressions for the states' beliefs are obtained by Bayes' rule and by substituting the expressions for the equilibrium strategies that have just been derived.

A sequential equilibrium will exist as long as the expressions for $e^*_{\mathrm{I}}(0)$, $e^*_{\mathrm{I}}(1)$, $e^*_{\mathrm{II}}(1)$, and $e^*_{\mathrm{II}'}(2)$ satisfy the constraints $0 \leqslant e^*_{\mathrm{I}}(0) \leqslant 1$, $0 \leqslant e^*_{\mathrm{I}}(1) \leqslant 1$, $0 \leqslant e^*_{\mathrm{II}}(1) \leqslant 1$, and $0 \leqslant e^*_{\mathrm{II}'}(2) \leqslant 1$. The first two constraints are satisfied because $0 < H < 1$. Only $H < 1$ is binding and this gives the restriction on δ in Proposition 1. The expression for $e^*_{\mathrm{II}}(1)$ is always less than one, and $e^*_{\mathrm{II}}(1) \geqslant 0$ yields the lower bound on ϱ for Equilibrium 2 in Proposition 1. Finally, examining the expression for $e^*_{\mathrm{II}'}(2)$ after substituting the expression for $e^*_{\mathrm{II}}(1)$ gives the upper bound.

Now consider the second subcase in which $\underline{H} \leqslant e_{\mathrm{I}}(0) < H$. If $H < 1$, Equilibrium 1 results. If $H > 1$, Equilibrium 4 obtains. The event $H = 1$ has measure zero and is disregarded.

Assume $\underline{H} \leqslant e_{\mathrm{I}}(0) < H < 1$. From II's best-reply correspondence, $e_{\mathrm{II}}(1) = 0$. If, moreover, $\underline{H} < e_{\mathrm{I}}(0)$, then the best reply for II' is $e_{\mathrm{II}'}(1) = 1$. But because $0 < \underline{H} \leqslant e_{\mathrm{I}}(0) < H < 1$, I is mixing at $\theta_{\mathrm{I}}(0)$. As has already been shown, $0 < e_{\mathrm{I}}(1) < 1$ for $\underline{H} \leqslant e_{\mathrm{I}}(0) < 1$, so I is also mixing at $\theta_{\mathrm{I}}(1)$. This means that I must be indifferent to the payoffs to exploiting and to not exploiting the situation. With $e_{\mathrm{II}}(1) = 0$ and $e_{\mathrm{II}'}(1) = 1$, this requires $q_{\mathrm{I}} = \varrho w_{\mathrm{I}} + (1 - \varrho)[(1 - f)(w_{\mathrm{I}} - d_{\mathrm{I}}) + f(w_{\mathrm{I}} - s_{\mathrm{I}})]$. This, however, is an event of measure zero. Disregarding it leaves $e_{\mathrm{I}}(0) = \underline{H}$.

Substituting for $\underline{H}$ gives the expression for $e^*_{\mathrm{I}}(0)$ for Equilibrium 1 in Proposition 1. Substituting the expression for $e^*_{\mathrm{I}}(0)$ in the constraint that defines Case 1 gives $e^*_{\mathrm{I}}(1)$. Finally, $e^*_{\mathrm{II}'}(2)$ is obtained by not-

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ing that $e_{II'}(2) = \underline{L}$ for all points in the intersection of Σ and BR for $e_I(0) < H$. Thus, $e^*_{II'}(2) = \underline{L}$.

The only strategy left to be specified is $e_{II'}(1)$ which is no longer necessarily 1 as it was when $e_I(0) > \underline{H}$. With $e_I(0) = \underline{H}$, II' may mix at $\theta_{II'}(1)$. To obtain $e^*_{II'}(1)$, note that because I mixes at $\theta_I(0)$ and $\theta_I(1)$ and $e_{II}(1) = 0$, $q_I = w_I - (1 - \varrho)\{e^*_{II'}(1)w_I + [1 - e^*_{II'}(1)][fd_I + (1 - f)s_I]\}$. Solving this for $e_{II'}(1)$ gives the expression for $e^*_{II'}(1)$ for Equilibrium 1 in Proposition 1.

Combining Bayes' rule and the expressions for the states' strategies gives the beliefs reported in Proposition 1. The constraint that $1 \geqslant \underline{H}$ yields the restriction on δ. Constraining the expressions for $e^*_I(0)$ and $e^*_{II'}(1)$ to be less than 1 gives the restriction on ϱ.

If $H > 1$, Equilibrium 4 results. With $H > 1$, if $\underline{H} < e^*_I(0) = 1 < H$, $e^*_{II}(1) = 0$, $e^*_{II'}(1) = 1$, $e^*_{II'}(2) = \underline{L}$, and $e^*_I(1) = [1 - \delta)/\delta][B_{II'}(2)/[1 - B_{II'}(2)]]$.

Beliefs are given by Bayes' rule and by substituting the expressions for the states' strategies. The constraint $H > 1$ yields the upper bound on δ, and the expression for $e^*_I(1)$ gives the lower bound. Because $e^*_I(0) = 1$, the expected payoff to exploiting the situation must be at least equal to the payoff to not exploiting the situation. This gives the restriction on ϱ for Equilibrium 4 in Proposition 1.

Case 2

Case 2 is defined by:

$$e_I(1)e_I(0) > [(1 - \delta)/\delta]\{B_{II'}(2)/[1 - B_{II'}(2)]\}$$

In this case, the best-reply correspondence for $e_{II'}(2)$ implies $e^*_{II'}(2) = 1$. This leaves $e^*_I(0)$, $e^*_I(1)$, $e^*_{II}(1)$, and $e^*_{II'}(1)$ to be specified.

The specification begins with two observations. First, because the resolve of II' is resolute, and with $e_{II'}(2) = 1$, I's best seem intuitively that if $e^*_{II}(1) > 0$, then $e^*_{II'}(1) = 1$. Algebra shows that the value of $\beta_{II}(1)$ that sustains $e_{II}(1) > 0$ is strictly greater than the value of $\beta_{II'}(1)$ that sustains $e_{II'}(1) = 1$. But, $\beta_{II}(1) = \beta_{II'}(1)$, so if $e_{II}(1) > 0$, then $e_{II'}(1) = 1$.

The second remark is that in a crisis equilibrium it must be that $e^*_{II}(1) > 0$. Suppose the contrary, that is, $e^*_{II}(1) = 0$. In a crisis equilibrium, there must be some chance of resistance, so $e^*_{II'}(1) > 0$. But given these strategies, resistance unambiguously signals that the challenged state is resolute, and with $e_{II'}(2) = 1$, I's best reply is $e_I(1) = 0$. This, however, contradicts the inequality defining Case 2. Thus, $e^*_{II}(1) > 0$. These remarks imply that $e^*_{II}(1) > 0$ and $e^*_{II'}(1) = 1$.

Now consider the problem facing I at $\theta_I(1)$. Let $B_I(1)$ be the value of $\beta_I(1)$ that leaves I indifferent to escalating and to submitting. Then $B_I(1) = [1 - (1 - 2f)(1 - 3f)](1 - R_I)/\{(1 - 2f)[(1 - 3f)R_I + 3f]\}$. Thus, I's best response at $\theta_I(1)$ is $e_I(1) = 1$ if $\beta_I(1) > B_I(1)$; $e_I(1) \in [0,1]$ if $\beta_I(1) = B_I(1)$; and $e_I(1) = 0$ if $\beta_I(1) < B_I(1)$.

Applying Bayes' rule and substituting for $\beta_I(1)$ implies that I's best-reply correspondence can be written as $e_I(1) = 1$ if $e_{II}(1) > [(1 - \varrho)/\varrho]\{B_I(1)/[1 - B_I(1)]\}$; $e_I(1) \in [0,1]$ if $e_{II}(1) = [(1 - \varrho)/\varrho]\{B_I(1)/[1 - B_I(1)]\}$; and $e_I(1) = 0$ if $e_{II}(1) < [(1 - \varrho)/\varrho]\{B_I(1)/[1 - B_I(1)]\}$.

The next strategy to be specified is $e^*_{II}(1)$. By the inequality defining Case 2, $e_I(1) > 0$. It will now be shown that $e_I(1) < 1$. Assume that $e_I(1) = 1$. Then $e_{II}(1) \geqslant (1 - \varrho)B_I(1)/\{\varrho[1 - B_I(1)]\}$. If, moreover, $e_I(1) = 1$ and $e^*_{I'}(1) = 1$, then II will submit at $\theta_{II}(1)$ because there is no chance of its prevailing. But if $e_{II}(1) = 0$, then $e_{II}(1) < (1 - \varrho)B_I(1)/\{\varrho[1 - B_I(1)]\}$, a contradiction. This contradiction leaves $e^*_I(1) < 1$. The fact that $0 < e^*_I(1) < 1$ and the best-reply correspondence for I at $\theta_I(1)$ yields $e^*_{II}(1) = (1 - \varrho)B_I(1)/\{\varrho[1 - B_I(1)]\}$.

The strategies left to be specified are $e^*_I(0)$ and $e^*_I(1)$, and this begins by obtaining a relation between them. For there

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to be an equilibrium $0 \leqslant e^*_{II}(1) = (1 - \varrho)B_I(1)/\{\varrho[1 - B_I(1)]\} \leqslant 1$. That is, $\varrho \geqslant B_I(1)$. Disregarding the event $\varrho = B_I(1)$ because it has measure zero and taking $\varrho > B_I(1)$, then $0 < e^*_I(1) < 1$. With $0 < e^*_{II}(1) < 1$, II is indifferent to escalating and submitting at $\theta_{II}(1)$. So

$$s_{II} = \beta_{II}(1)\,[fd_{II} + (1 - f)\,([1 - e^*_I(1)]\,w_{II} + e^*_I(1)\,\{2f + (1 - 2f)s_{II}\}) + [1 - \beta_{II}(1)][2f + (1 - 2f)s_{II}]$$

Substituting for $\beta_{II}(1)$ yields

$$e^*_I(1) = 1 - \left[1 + \left(\frac{1 - \delta}{\delta}\right)\frac{1}{e_I(0)}\right]\left(\frac{[1 - (1 - f)(1 - 2f)][1 - R_{II})}{(1 - f)[(1 - 2f)R_{II} + 2f]}\right)$$

This relation is the same as the relation between $e_I(0)$ and $\hat{e}_I(1)$ that was derived in Case 1. In Case 2, the sequential equilibria must lie along $\hat{e}_I(1)$ in Figure A-1 above the constraint Z. This implies $0 < e^*_I(1) < 1$.

I's best-response correspondence at $\theta_I(0)$ will now be determined. Note that because I randomizes at $\theta_I(1)$, I's expected payoff if the game reaches this information set is s_I. Then letting Q_I be the value of the status quo at which I would be indifferent to exploiting the situation and accepting the status quo gives

$$Q_I = \varrho\{[1 - e^*_{II}(1)]w_I + e^*_{II}(1)\,[fd_I + (1 - f)s_I]\} + (1 - \varrho)\,\{[1 - e^*_{II'}(1)]w_I + e^*_{II'}(1)[fd_I + (1 - f)s_I]\}$$

Substituting the expressions for $e^*_{II}(1)$ and $e^*_{II'}(1)$ produces

$$Q_I = \{[\varrho - B_I(1)]/[1 - B_I(1)]\}\,[(1 - f)(w_I - s_I) + f(w_I - d_I)] + fd_I + (1 - f)s_I$$

The best-reply correspondence for I at $\theta_I(0)$ is, therefore, $e_I(0) = 1$ if $q_I < Q_I$; $e_I(0) \in [0,1]$ if $q_I = Q_I$; and $e_I(0) = 0$ if $q_I > Q_I$.

The final step in determining $e^*_I(0)$ and $e^*_I(1)$ is to note that if there is to be a sequential crisis equilibrium, then $q_I \leqslant Q_I$. Otherwise, I will never challenge the status quo. A challenge will then unambituously signal that the challenger is resolute. This challenger escalates with probability 1 for all $n \leqslant N_{I'}$. There will be no resistance and no crisis. Hence, $q_I \leqslant Q_I$.

Disregarding the event $q_I = Q_I$ which has measure zero, the equilibrium strategies $e^*_I(0)$ and $e^*_I(1)$ may now be specified. If $q_I < Q_I$, then $e^*_I(0) = 1$ and $e^*_I(1)$ is the value of $\hat{e}_I(1)$ evaluated at $e_I(0) = 1$, which is the expression for $e^*_I(1)$ in Proposition 1.

Expressions for the relevant beliefs are obtained with Bayes' rule and by substituting the expressions for the states' strategies.

Several conditions are needed to ensure the existence of the sequential crisis equilibrium in Case 2. First, $\hat{e}_I(1)$ must be above the constraint Z at $e_I(0) = 1$. This is equivalent to assuming $H < 1$, which, when solved for δ, gives the restriction stated in Proposition 1. The second condition is that $q_I < Q_I$, which means that $\varrho > 1 - \{[1 - \beta^*_I(1)](w_I - q_I)\}/\{(w_I - d_I)[(1 - f)R_I + f]\}$. When this second condition is met, so is the assumption made above that $\varrho > B_I(1)$. The final conditions are that $e^*_I(1) \leqslant 1$ and $e^*_{II}(1) \leqslant 1$. But $H < 1$ assures $e^*_I(1) < 1$ and $\varrho > B_I(1)$ that $e^*_{II}(1) < 1$. QED

A final observation is in order. Note that when $\delta = 1$ the model reduces to that investigated in Powell 1987, in which there was only one-sided incomplete information in that only the potential challenger was uncertain of the level of resolve of its adversary. As to be hoped and expected, the equilibria found above degenerate to the unique equilibrium

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found with one-sided incomplete information. With $\delta = 1$, Equilibrium 4 does not exist. Equilibria 1 and 2 disappear because with $\delta = 1$, $e^*_{\mathrm{I}}(0) = 0$. This makes it impossible to satisfy the inequality defining Case 1. Finally, for $\delta = 1$, the expressions describing Equilibrium 3 reduce to those in Powell 1987.

Notes

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1. For a discussion of this means of exerting coercive pressure see Kaplan 1962. For a comparison of this means of exerting pressure to the manipulation of the risk of an unlimited nuclear exchange, see Powell 1985 and Schelling 1962.

2. For a discussion of the various ways that resolve has been formalized, see Powell 1987 and Wagner 1982.

3. See Kreps and Wilson 1982 for a definition and more detailed discussion of the properties of sequential equilibria.

4. If δ and ϱ do not satisfy the constraints defined in Proposition 1, then no sequential crisis equilibria exist. The only sequential equilibria that exist are not also crisis equilibria.

5. Because both I and I′ always submit for $n > N_{\mathrm{I}'}$ and both II and II′ always submit for $m > N_{\mathrm{II}'}$, the beliefs off the equilibrium path at $\theta_{\mathrm{I}}(n)$ and $\theta_{\mathrm{I}'}(n)$ and at $\theta_{\mathrm{II}}(m)$ and $\theta_{\mathrm{II}'}(m)$ can be anything. These arbitrary beliefs, however, do not affect the distribution of the game's outcomes and in this sense are irrelevant.

6. In other words, the states' levels of resolve are sufficient statistics for their payoffs.

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Robert Powell is Assistant Professor of Government, Harvard University, Cambridge, MA 02138.

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CRISIS LEARNING GAMES

RUSSELL J. LENG
Middlebury College

Findings from a data-based study of bargaining in recurrent crises between evenly matched states provide the foundation for the construction of four crisis-learning games. Symmetrical and asymmetrical nuclear and nonnuclear sequential three-by-three games assuming complete information and nonmyopic play are presented and analyzed. The empirical study indicated that states that were unsuccessful in one crisis were likely to move to more coercive bargaining strategies in the next crisis. The four sequential games offer insights as to why this is likely to produce unwanted consequences, while demonstrating the importance of the participants' initial strategy choices. With the realpolitik lessons suggested by the earlier study removed, the dynamics of the games present a case for beginning with a cooperative strategy and moving to reciprocating, or tit-for-tat, bargaining.

Game theorists (Brams 1985; Kilgour and Zagare 1987; Powell 1987; Zagare 1985) have devoted considerable attention to bargaining in interstate security crises, particularly in nuclear crises. But an important related problem—the effects of changes in the strategy choices of policymakers from one crisis to the next—has been largely ignored. I examine that problem through four crisis-learning games constructed from findings drawn from a data-based study of bargaining in recurrent crises (Leng 1983).

The data-based study was concerned with two questions: What are the lessons that policymakers draw from one crisis experience to guide their behavior in the next crisis? and What are the likely consequences of applying those lessons?

Previous Research

Regarding the first question, common sense suggests that the success or failure of the bargaining strategy employed in one crisis is likely to influence the choice of bargaining strategy in the next crisis. Jervis (1976) has hypothesized that a tendency to see their own state's behavior as decisive encourages policymakers to consider unsuccessful outcomes as failures of policy so that they are likely to react by switching to a different bargaining strategy in the next encounter. The same reasoning encourages them to repeat bargaining strategies associated with successful outcomes.

Jervis leaves open the question of *what sort* of change to expect if the outcome of the preceding crisis was unsatisfactory. I have hypothesized elsewhere (Leng 1983), that national policymakers are most likely to shift to a more coercive bargaining strategy. This proposition is based on the pervasiveness of a realpolitik tradition that views interstate crises as dominated by considerations of power politics and prescribes bargaining strategies that demonstrate power and resolve. Empirical support for this hypothesis appears in recent studies (Leng and Wheeler 1979;

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Snyder and Diesing 1977) demonstrating the pervasiveness of coercive bargaining across a wide range of twentieth century crises.

These hypotheses were combined in a "realpolitik experiential learning" (REL) model of bargaining behavior in recurrent crises (Leng 1983). The REL model begins with the proposition that national policymakers assume that the outcome of the preceding crisis was related directly to the bargaining strategy that they employed. The experiential learning component of the proposition posits that policymakers will repeat those strategies that led to victory and change those that led to unsatisfactory outcomes. The realpolitik component of the proposition posits that policymakers are likely to assume that the undesirable outcome was the result of a failure credibly to demonstrate resolve (Leng 1983, 380–82). This leads to a prescription to employ a more belligerent bargaining strategy in the next crisis with the same adversary.

The same realpolitik reasoning that prescribes the switch to a more belligerent strategy by the loser encourages the previous winner to respond in kind once it observes the change in its adversary's behavior. If both parties are dissatisfied with the outcome of the preceding crisis, the REL proposition posits that each will begin the next with a more coercive bargaining strategy. Thus, in all instances except those when the two parties reached a mutually satisfactory compromise in the preceding crisis, REL predicts that each succeeding crisis between the two adversaries will become more belligerent. A test of these hypotheses (Leng 1983), employing events data from 18 twentieth century crises involving six pairs of nations of relatively equal military capability yielded findings supporting the REL propositions. The study's answer to the second question—What are the likely consequences of following the lessons that policymakers draw from crisis experiences?—is that each successive crisis will become more dangerously conflictual.

The Research Question

The findings from the empirical study raise an important prescriptive question: How can policymakers avoid this trap? If coercion only begets coercion, is there a better approach to crisis bargaining in recurrent crises? Data-based studies of crisis bargaining more generally (Huth and Russett 1987; Leng 1984; Leng and Wheeler 1979), have indicated that "firm-but-fair," or tit-for-tat bargaining strategies are superior to escalating coercive strategies in crises between states relatively evenly matched in military capabilities and motivation. Axelrod (1984) has used prisoner's dilemma games to explore the strategic logic behind the efficacy of tit-for-tat strategies, and he offers a persuasive rationale for its effectiveness.

Game theory is employed here to trace the consequences of following an REL approach to the initial strategy choice in recurrent crises in which the players otherwise employ a strictly rational bargaining approach. The outcomes of these cases will be compared to those in which a rational bargaining strategy is followed from the outset. Then those outcomes will be compared to the outcomes obtained by following a simple tit-for-tat bargaining strategy. The results offer insights into the consequences of initial strategy choices, providing, as well, a provocative answer to the question of the best approach to bargaining in recurrent crises.

Game Theory and the REL Model

The REL model is the foundation for the construction of four sequential three-by-three crisis learning games in which the strategy options for each player are based on the strategy employed in the preceding crisis: (1) a repeat of the same strategy, (2) a change to a more cooperative

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strategy, or (3) a change to a more coercive strategy than either party employed in the preceding crisis. The label attached to each strategy denotes the degree of coercion or cooperation *relative* to that employed in the preceding crisis. A "more cooperative" bargaining strategy, for example, may not appear to be very cooperative when viewed in absolute terms.

The crisis is assumed to have been initiated through a precipitant event or situation, which has led to a confrontation between the two states (see Leng and Singer n.d.; Snyder and Diesing 1977, chap. 1). The initial moves represent the opening bargaining strategies of the two governments. The basic REL proposition that the crisis begins with the loser of the preceding crisis choosing a more coercive strategy and the winner of the preceding crisis repeating the same strategy is retained. When the preceding crisis ended unsatisfactorily for both parties, the game begins with both parties choosing a more coercive strategy. These moves produce the initial outcome, which determines the starting point for a sequential game in which the two players make alternating moves until the game reaches a final outcome.

Allowing for the possibility of states breaking out of the REL trap, however, requires that once the initial strategies are selected, the requirement to respond in kind to coercive moves be dropped to permit the players to assess their bargaining strategies on a purely rational basis, that is, in terms of maximizing their pay-offs. Strictly following the REL proposition would produce a trivial game with the two sides becoming locked in to more coercive strategies on the second move.

Four types of crisis-learning games are identified, based on two distinctions: nuclear and nonnuclear crises, and asymmetric and symmetric pay-off structures. It is assumed that being locked in to mutually coercive strategies on the road to war represents the worst possible outcome in a nuclear crisis, but not in a nonnuclear crisis. The pay-offs also differ according to whether the outcomes of the preceding crises were the same for both parties (symmetric) or there was a winner and a loser (asymmetric). I will outline the structure and rules of the games, then discuss the sequence of moves in each of the four types.

Structure and Rules of the Game

Strategies

There are three possible strategies for each player: C = a more cooperative bargaining strategy than that used by either player in the preceding crisis, S = the same strategy used by that player in the preceding crisis, and E = a more coercive strategy than that used by either player in the preceding crisis.

Outcomes

The REL model posits that the players will make the following assumptions about the relationship between the strategies and the outcomes:

ES or EC = diplomatic victory for strategy-E player
SC = diplomatic victory for strategy-S player
SS = same outcome as the preceding crisis
CC = mutually satisfactory compromise
EE = war

The first two assumptions are based on the realpolitik notion that demonstrating greater resolve and toughness prevails over a more accommodating opponent. The SS outcome represents a repeat of the last crisis outcome. CC is a mutually satisfactory compromise. The EE pay-off occurs when the players are locked in to mutually coercive strategies; that is, if the

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Table 1. Asymmetrical Nonnuclear Game

Row	Column C	Column S	Column E
C	5,6	3,9	2,8
S	9,4	8,2	1,7
E	7,3	6,1	4,5

player with the next move does not depart from an EE outcome, the game ends in war.

Players' Goals

The primary objective of each player is to maximize its self-interest. In a nonnuclear crisis, winning is preferred over compromise, compromise over war, war over submission. In a nuclear crisis, the preference ordering is: winning, compromise, submission, war. The secondary objective is to minimize the level of coercion employed. Two assumptions lay behind the secondary objective: (1) the use of coercion increases the risk of war, so it is best to win at the lowest possible level of risk; (2) if it is necessary to compromise, or to submit, the costs increase relative to the extent to which the player has invested its reputation for resolve in an unsuccessful coercive strategy.

Preference Orderings

The players' outcome preferences vary according to which of the four games is being played. In the asymmetric nonnuclear crisis, for example, where Row was the winner of the preceding crisis and Column was the loser, the outcome preference orderings would be:

Row's preferences:
1. Win SC > SS > EC > ES
2. Compromise CC > EE
3. Submit CS > CE > SE

Column's preferences:
1. Win CS > CE > SE
2. Compromise CC > EE >
3. Submit SC > SE > EC > ES

(The symbol " > " is interpreted as "preferred over.") The result is the three-by-three ordinal game exhibited in normal form in Table 1. The first entry in each cell represents the pay-off to Row, the second entry is Column's pay-off. The outcome pay-offs are ranked from *most preferred* to *least preferred*, with a score of nine representing the most preferred outcome. In asymmetric games, Row represents the winner of the preceding crisis and Column represents the loser.

Play of the Game

The initial outcome of the game is determined in accordance with the strategies predicted by the REL model. Asymmetric games begin with Row (winner of the preceding crisis) choosing the same strategy (S) as that employed in the last crisis, and Column (loser of the preceding crisis) moving to a more coercive strategy (E). In a symmetric game, where both sides were dissatisfied with the outcome of the preceding crisis, each player chooses strategy E. If they both were satisfied with the preceding outcome, each would choose strategy S. In the asymmetric game shown in Table 1, the initial outcome is at (1,7).

Row is given the first opportunity to move from the initial outcome. This is based on the REL assumption that, in asymmetric games, the "winner" of the preceding crisis must decide how to react to the changed strategy of the "loser" of the preceding crisis. In symmetric games either player may move first; the interpretation of the dynamics of the game remain the same.

From this point on, the rules of play are based on the theory of moves (Brams and Wittman 1981) for sequential games. If

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Row chooses to change its strategy and move to a new outcome, then Column must decide whether to stay at that outcome, thereby ending the game, or to switch its strategy and move to a new outcome. The moves continue to alternate until the player whose turn it is decides to stay at the current outcome.

What constitutes a rational choice—move or stay—at each outcome depends on whether the game is viewed myopically or nonmyopically. In a game played myopically, each player simply asks whether there is a better outcome that can be obtained on the next move. If the game is played nonmyopically, each player has the foresight to calculate its moves based on the knowledge of the other player's response, its counterresponse, and so on, in an extended series of strictly alternating moves. Thus, each move is made in light of its long-term consequences. This is a more accurate depiction of the reality of crisis decision making, where efforts are made to "look down the road" to gauge the consequences of action-response sequences. Just *how far* down the road real world policymakers can, and do, look in the course of an ongoing crisis is an open question. I do not attempt to answer that question here; however, in the games that follow, I will note the degree of farsightedness necessary to achieve stable nonmyopic outcomes. This will afford some sense of just how farsighted decision makers must be to make the best strategy choices.

The assumption of nonmyopic play has important implications for how the game will end. Brams and Wittman (1981) have extended notions of stability and equilibrium to include nonmyopic calculation. Kilgour (1985) has extended and broadened these concepts to base them on the game's final outcome.[1] Using Kilgour's (1985) approach, the current outcome is *nonmyopically stable* for the player concerned unless it can do better by departing to set in motion a series of moves and countermoves that will yield a preferable final outcome. If an outcome is nonmyopically stable for both players, it is a *nonmyopic equilibrium*. Kilgour's approach is particularly apt for our purposes. National leaders often accept crisis outcomes that fall short of total success in order to terminate increasingly intolerable situations or to avoid potentially disastrous outcomes.

Before turning to a discussion of the four crisis-learning games, the reader should be alerted to some of the limitations in the crisis models as they are presented. First, all of the assumptions in the study are limited to crises between states that are relatively evenly matched in usable military capabilities and their perceptions of the gravity of the values at stake. This is not an uncommon phenomenon in recurrent crises, but it does exclude those cases where there are significant differences in capabilities or motivation. More problematic is the assumption that each of the players possesses complete information regarding the preferences of the other player. Real-world policymakers must make subjective estimates of the preference orderings of the other party; in fact, part of the bargaining process is designed to discover those preferences (See Snyder and Diesing 1977, chap. 2). The assumption of complete information is a simplification designed to clarify the essential components of the games. The transformation of the models to games of incomplete information, however, is a straightforward process. An attempt also has been made to alert the reader at those points, as in the discussion of "brinkmanship," where the assumption of complete information becomes especially problematic.

Finally, there is the very simplicity of the strategic choices and outcomes presented in the game matrices. Obviously, the range of choices and outcomes facing real-world policymakers is more complex. The simple typology of choices parallels

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those presented in the data-based study (Leng 1983). As such, it depicts the most salient aspects of the crisis-learning model, rather than a richly descriptive image of real-world crisis bargaining. A more complex representation might be appropriate in later refinements of the model, but at this stage of the research it could obscure the fundamental relationships of interest.[2]

Beginning with the asymmetric nonnuclear crisis-learning game depicted in Table 1, I will describe the sequence of moves and outcomes in which the initial moves are based on the REL prescription.

Four Crisis-Learning Games

Asymmetrical Nonnuclear Game

The game in Table 1 assumes that the preceding crisis ended in a diplomatic victory for Row and a diplomatic defeat for Column. As noted above, the outcomes are ranked according to the players' preference orderings, with nine representing the most-preferred outcome and one the least-preferred outcome. The players do not possess usable nuclear weapons; therefore each prefers war (EE) over losing to the other player.

The simultaneous opening moves place the game at (1,7). Row's choices are to move to (2,8) or (4,5), or stay at (1,7). The rational choice is to move to (4,5). The (4,5) outcome is not only a Nash equilibrium, which would mark the end of the game if it were played myopically, it is also a nonmyopic equilibrium as defined by Kilgour (1985); that is, even with the capacity to look ahead to the final outcome, neither player could improve its position by departing from (4,5). Nor could Row have done better with a different choice at (1,7). Staying at (1,7) would yield Row's worst outcome. If Row moved to (2,8), a nonmyopic Column would stay at that outcome and obtain its second-best pay-off. (Moving to [3,9] would allow Row to move to [8,2], etc.) Played rationally by both players, the game ends at (4,5) if it is played either myopically or nonmyopically.

In this game it is easy to play through the possibilities to see the final nonmyopic outcome, but that is not always the case; therefore, the sequences of nonmyopic play for each of the four games has been computed by machine, employing a program developed by Kilgour (1985).

The move by Row to (4,5) is consistent with REL reasoning, that is, that a more coercive strategy by Column is most likely to evoke a response in kind from Row. The two sides then become locked in to mutually coercive strategies that cause the crisis to escalate to war. It is not hard to envision real-world illustrations of such a pattern. A classic example would be the Russians (diplomatic losers to the Austrians in two crises in the Balkans) and Austria on the eve of World War I. Given the pay-offs in Table 1, the war (4,5) outcome can be avoided only if both players choose C as their initial strategies. That would contradict assumptions of the REL model, but it does suggest the critical nature of the initial strategy choices, an issue to which I will return.

Symmetrical Nonnuclear Game

The symmetrical game occurs when the previous crisis ended in a compromise, stalemate, or war.[3] We will begin the examination of this type with the assumption that neither player was satisfied by the outcome, so that each is inclined to attempt a more coercive strategy in the next crisis. There is no identifiable winner or loser, so the preference orderings of the players are mirror images of each other, with equal pay-offs for CC, SS, and EE, as shown in Table 2. Either player can make the first move from the initial outcome. From there on, the moves alternate as in the asymmetrical game.

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In those cases where both sides were dissatisfied with the outcome of the previous crisis, each starts with strategy E, so that the initial outcome is (4,4). It is also the final outcome, as (4,4) is a Nash equilibrium and nonmyopic equilibrium, signifying the termination of either a myopic or nonmyopic game. In fact, if *either* player chooses E as its beginning strategy, the game will end at the EE outcome. The outcome highlights the consequences of the only nonrational component of the game—the initial moves based on REL reasoning. Unhappy with the outcome of the preceding crisis, perhaps goaded on by publics demanding more assertive action, policymakers on both sides turn to initially more coercive bargaining strategies that result in swift escalation to war. A good illustration of this phenomenon occurred in the Indo-Pakistani crisis preceding the Second Kashmir War in 1965. It had been preceded by a cease-fire and diplomatic stalemate in 1948 that left both sides frustrated. When the smoldering conflict rekindled into a crisis in 1965, it swiftly escalated to war.

If both players *were* satisfied with the outcome of the previous crisis, so that each repeated its earlier strategy (S), the initial outcome would be (5,5). Played myopically, the game still ends at (4,4), but if it is played nonmyopically the final outcome becomes (5,5); that is, if Row has enough foresight to look ahead just one move, it will resist the move to (7,1), recognizing that Column will respond by moving to (4,4); instead, Row will stay at (5,5), which is a nonmyopic equilibrium.

A third nonmyopic equilibrium, which is Pareto-superior, occurs at (6,6), but this obtains only if both players begin the game with C strategies, a condition that runs contrary to the assumptions of the REL model. In fact, provided that neither player begins the game with strategy E, the game will yield at least the (5,5) outcome.

Table 2. Symmetrical Nonnuclear Game

	Column		
Row	C	S	E
C	6,6	3,9	2,8
S	9,3	5,5	1,7
E	8,2	7,1	4,4

Asymmetrical Nuclear Game

The asymmetrical nuclear game that appears in Table 3 starts from the same preference orderings as the asymmetrical nonnuclear game, except that the EE outcome (nuclear war) now represents the worst crisis outcome for either player.

The game begins at (2,7), with previous winner Row using the same strategy as in the preceding crisis (S), and previous loser Column using a more coercive strategy (E). Played myopically, this game cycles through the upper right-hand corner of the matrix in a counterclockwise fashion. Row chooses (3,8) over either (1,1) or staying at (2,7), prompting Column to move to (4,9); Row moves to (8,3), and Column moves back to (2,7) to restart the cycle. The game ends in a stalemate, a frustrating, but safe, outcome.

This would also be the outcome of a nonmyopic game if we interpreted a move by either party to (1,1) to result in nuclear war, as the rational players would avoid that suicidal outcome. I interpret a move to (1,1), however, not as the outbreak of

Table 3. Asymmetrical Nuclear Game

	Column		
Row	C	S	E
C	5,6	4,9	3,8
S	9,5	8,3	2,7
E	7,4	6,2	1,1

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nuclear war but as a commitment to go to war (in this case, nuclear war), failing a more cooperative move by the other party; just as a move to (6,6) would represent a commitment to a compromise, provided the other party did not move away from that outcome.[4] Following the rules of a sequential game, the outcomes obtain only if the player with the option to depart from the outcome decides to stay. Under these circumstances, the best choice for Row is to move from (2,7) to (1,1), thereby forcing Column to choose between staying at (1,1) or moving to (6,2) or (7,4). A rational Column will depart and, after seven moves, the game ends at (9,5).

Row's move to (1,1) to turn a stalemate into victory, suggests an act of "brinkmanship," that is, the creation of a shared risk of nuclear war, with the responsibility for avoiding the mutual disaster shifted to Column. In fact, the asymmetrical nuclear game offers some interesting insights into real-world nuclear crises, but it also demonstrates some of the limitations of viewing real world crises as sequential games of complete information, particularly with regard to the use of brinkmanship.

Played myopically, the asymmetric nuclear game bears some resemblance to the Berlin Wall crisis of 1961, with Row representing the United States and its Western allies, and Column representing the Soviet Union. The initial Soviet strategy was decidedly coercive (an ultimatum on Western rights in West Berlin); whereas the West began by expressing its determination to defend its access to Berlin, much in the manner of the 1948 crisis. The 1961 crisis then cycled back and forth between cooperative and coercive moves by both sides. The United States did not resort to an act of brinkmanship to break out of the cycle, and the crisis finally died down without any settlement. Most observers saw the outcome as falling somewhere between a stalemate and a Soviet victory.

The nonmyopic version of the game, which includes the move through (1,1), raises the issue of brinkmanship. Not coincidentally, the United States did employ brinkmanship in its next crisis with the Soviet Union in 1962. The Cuban crisis has been subjected to innumerable analyses, and Brams (1985, 47–62) has demonstrated how different hypotheses regarding the key decision makers' perceptions of the pay-offs associated with alternative strategies can lead to significantly different interpretations of the game. It is, nonetheless, worth a digression here, to consider the implications of the use of brinkmanship, and some of the descriptive limitations of a sequential game played with complete information.

The asymmetrical nuclear game grants the brinkmanship option to the party in the most defensive position, that is, the party that must respond to the coercive initiative of the other. This is consistent with the notion that the defender of the status quo has an edge in motivation that makes resort to brinkmanship on its part a more credible strategy.[5] In fact, in the Cuban missile crisis, virtually all of the elements in the balance of motivation favored the U.S. side: the United States was defender of the status quo, its security was more immediately threatened, and it was the relative loser of the last crisis.[6] The United States could make a case for resorting to brinkmanship that could not be made by the Soviets, nor could it have been made by the United States in the 1961 Berlin crisis, when the United States would have been on the light side in both motivation and usable military capability.

The sequential game with complete information, however, disguises other aspects of brinkmanship that contribute to the enormous risks it entails. When Row moves to (1,1) in the crisis game, it shifts the responsibility to Column for triggering or avoiding a nuclear war. This is not inconceivable in the real world; in fact, Schelling (1966) has argued that

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Kennedy did just that when he publicly committed the United States to defend the quarantine during the Cuban missile crisis. But Kennedy's commitment was not truly irrevocable, and it is hard to imagine instances in real-world crises where either party would deliberately close off all possibilities of retreating from the brink of nuclear war. Moreover, there is always the risk that one party will not move because it assumes that the other is bluffing. In the sequential game, Column must make the next move or trigger nuclear war. The assumption of complete information means Row knows that and also knows that Column will move rather than accept its least-preferred outcome. That, of course, is not true in the real world where policymakers considering an act of brinkmanship must make subjective estimates of the other party's preferences, and of the other party's estimates of their preferences. Finally, there is the question of how much time the parties have before it is too late to move from (1,1).

It would not be difficult to alter the model to reflect these uncertainties. A straightforward approach would be to treat the situation as a game of incomplete information, with probability estimates of the likelihood that the player with the opportunity to move from (1,1)—or both players if one wished to modify the theory of moves to give each the opportunity to depart from (1,1)—would actually do so. A random element could be added to the calculation to include the possibility of the players running out of time, or accidentally "slipping over the brink." Modifications along these lines would more accurately model the concept of "shared risk" that lies behind the notion of brinkmanship (see Powell 1988). Such refinements would be appropriate at a later stage of the development of the models; at this point, however, they would detract from one of my basic objectives—to illustrate the effects of initial moves on the dynamics of the games, even when the players are equipped with the farseeing rationality that obtains in a nonmyopic game with complete information.

Table 4. Symmetrical Nuclear Game

	Column		
Row	C	S	E
C	6,6	4,9	3,8
S	9,4	5,5	2,7
E	8,3	7,2	1,1

Symmetrical Nuclear Game

The game matrix for a symmetrical nuclear learning game, in which the players hold the same views regarding the outcome of the preceding crisis, appears in Table 4.

The game is most interesting in those cases where both parties were unhappy with the outcome of the preceding crisis. With both players turning to more coercive bargaining strategies, the initial outcome is at (1,1). The game cycles if it is played myopically; however, this avoids the nuclear war outcome, with the crisis ending in a stalemate. For example, if Row makes the first move away from (1,1), the outcomes will be (3,8), (4,9), (7,2), (8,3), (9,4), (2,7), (3,8), and so on.

Played nonmyopically, the game has an intriguing property. The first player to depart from (1,1) is at a disadvantage. If Row moves to (3,8), Column's best choice is to stay and end the game at that outcome. If Column has the first move and departs to (8,3), Row's best choice is to stay and end the game at that outcome. The potentially disastrous (1,1) outcome gives the symmetrical nuclear learning game properties similar to that of chicken.[7] As a sequential game, it nicely illustrates the dynamics of the original teenage game of chicken played with

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automobiles. The game begins with both players headed on a collision course; the first to swerve from the path to mutual disaster loses. The dynamics of chicken as a two-by-two sequential game have been analyzed extensively by Brams (1983, 1985), so the discussion here will be directed to its implications for the REL model.

As posited by the REL model, the initial outcome is at (1,1) because both parties, taking a realpolitik perspective, assume that the failure to achieve their goals in the previous crisis resulted from a failure to demonstrate adequately their resolve. Each begins the crisis determined to make the other side "blink" first. If both remain determined, the nuclear disaster occurs. If not, the first party to move to a more cooperative bargaining strategy loses. Increasing the credibility of one's commitment to stay at (1,1) increases the likelihood of winning if the opponent lacks resolve, but it also makes it more difficult for one to retreat if the opponent does not. As in the asymmetrical nuclear game, a more accurate depiction of the desperate risks involved in this game could be obtained by transforming it into a game of incomplete information, with each side forced to make probability estimates of the likelihood of the other party moving from (1,1), along with a random element to represent the danger of both parties waiting too long before moving. But for our immediate purposes, the lesson behind the game as it now stands is clear enough: the desperate situation is the result of where the game begins, that is, with each party choosing a more coercive initial bargaining strategy.

There are two nonmyopic equilibria in the game, at (5,5) and (6,6). The (5,5) equilibrium can be reached if neither player chooses strategy E as an opening move. The (6,6) outcome can be reached only if both players choose C as their opening strategy. As in the other four types of crisis-learning models, the players can achieve a mutually beneficial outcome only if the "lesson" from the preceding crisis that the REL model says is most likely to be learned—for dissatisfied players to begin with more coercive strategies in the next encounter—is rejected by both players.

Summary

The notion that an unsatisfactory outcome to the preceding crisis will encourage decision makers to adopt more coercive bargaining strategies in the next confrontation lies at the heart of the REL model of state behavior in recurrent crises. A data-based study (Leng 1983) of six pairs of nations in three successive crises each offered empirical evidence to support the descriptive validity of the proposition, while suggesting that it was a recipe for disaster.

The examination of the dynamics of the four crisis-learning games reinforces that impression. Even with the deterministic element of the REL model—which posits that resorting to a more coercive bargaining strategy by one party will cause the other to follow suit—removed, these are dangerous games. In each of the two nonnuclear games, a final outcome of escalation to war (EE) is inescapable if *either* party begins the game with a more coercive (E) bargaining strategy. The situation hardly improves in the two nuclear games. Played myopically, the asymmetrical nuclear game cycles to a stalemate. The stalemate can be avoided if the game is played nonmyopically but only through an act of brinkmanship on the part of Row. And the symmetric nuclear game begins on a collision course, with the first player to depart from EE the loser. In none of the games is the player who begins with a more coercive bargaining strategy than it employed in the previous crisis at an advantage.

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Finding the Best Strategy

What *is* the best opening strategy? To examine this question, we will drop the fixed opening moves prescribed by the REL model, along with the requirement that the previous winner have the first opportunity to move from the initial outcome. Then we will consider the final nonmyopic outcome for each possible opening move, assuming that either player can change its initial strategy and set in motion the sequence of alternating moves (Brams 1983, 75–79). If the final outcome varies according to which player departs from the initial outcome, two sets of final outcomes will be presented for that cell in the matrix, one for each player making the first move. In these cases, the final outcome with Row making the first move will be listed first.

Rational Bargaining

In effect, we will be considering the games strictly from the perspective of rational calculations, with the REL assumptions removed. The game begins with both players aware that they have become embroiled in another crisis and each choosing among the three possible strategy choices. What remains are the realpolitik assumptions at the heart of the pay-off matrix, with more coercive (E) strategies prevailing over repeat (S) strategies, and S strategies prevailing over changes to more cooperative (C) strategies.

Table 5 presents the original and final outcome matrices for game A, the asymmetric nonnuclear game.

The cell entries in the final outcome matrix in Table 5 indicate where the game would end, provided that the initial outcome was in that cell. The upper half of the table simply reproduces the initial outcome matrix appearing in Table 1.

The dominant strategy for either player in the asymmetric nonnuclear game is C, the more cooperative strategy. If both players choose C, the initial outcome would be (5,6), where each of the nonmyopic players would choose to stay. Any other strategy choice by either player would produce a final outcome of (4,5). If, for example, the game started at SC, or (9,4), Column would depart to (1,7), and Row would move to (4,5), where the game would end.

C is also the best initial strategy in game B, the symmetrical nonnuclear game. Its initial and final outcome matrices appear in Table 6.

Table 5. Asymmetrical Nonnuclear Game, Original and Final Outcome Matrices

Row	Column C	Column S	Column E
	Game Matrix		
C	5,6	3,9	2,8
S	9,4	8,2	1,7
E	7,3	6,1	4,5
	Final Outcome Matrix		
C	5,6	4,5	4,5
S	4,5	4,5	4,5
E	4,5	4,5	4,5

Table 6. Symmetrical Nonnuclear Game, Original Game Matrix and Final Outcome Matrix

Row	Column C	Column S	Column E
	Game Matrix		
C	6,6	3,9	2,8
S	9,3	5,5	1,7
E	8,2	7,1	4,4
	Final Outcome Matrix		
C	6,6	5,5	4,4
S	5,5	5,5	4,4
E	4,4	4,4	4,4

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Table 7. Asymmetrical Nuclear Game, Original Game Matrix and Final Outcome Matrix

Row	Column C	Column S	Column E
	Game Matrix		
C	5,6	4,9	3,8
S	9,5	8,3	2,7
E	7,4	6,2	1,1
	Final Outcome Matrix		
C	9,5	9,5	9,5
S	9,5	9,5	9,5
E	9,5–3,8	9,5–3,8	3,8–9,5

Choosing C offers either player the possibility of the highest possible outcome with no greater danger than any other initial strategy. Although this game has some of the properties of prisoner's dilemma, the dynamics of the sequential game eliminate the danger of being "suckered" from being too cooperative. On the other hand, if either player begins with strategy E, the game ends at EE.

The outcome matrix for game C, the asymmetrical nuclear game, appears in Table 7.

This game is more complex. Convergence on final outcomes is not reached until after seven moves, and there are three instances in which the final outcome varies according to which player is the first to depart from the initial outcome. For example, if the initial outcome is EC, Row can receive its most-preferred outcome (9) if it is the first to depart, and Column can receive its second favored outcome (8) if it departs first.

Row can assure its best outcome by choosing either strategy C or strategy S. C would be preferred based on the secondary objective of minimizing the amount of coercion necessary to achieve the objective. The choices for Column, loser in the last crisis, are more intriguing. The balance of probable outcomes is equal, but the ordering of the final outcomes across row E suggests an interesting property of the game. If Column chooses either C or S as an opening strategy when Row chooses E, then Column's success is dependent on making the first move away from the initial outcome to obtain a (3,8) final outcome. One could argue that in a real-world crisis, the expectation would be that Column would be the first to move, in order to respond to the challenge posed by Row's more coercive E strategy (Leng 1983; Snyder and Diesing 1977). That assumption, however, is not part of the game. A glance at the matrix suggests that both players have an incentive to be the first to depart from the initial outcome at either EC or ES.

If Column's initial strategy choice is C or S, its success or failure is at least in its own hands. On the other hand, if Column chooses strategy E, so that the initial outcome is at EE, Column's hopes for achieving its second-best pay-off are in the hands of Row. Row must depart first if Column is to avoid its fifth-best pay-off (9,5) or a disaster (1,1). Unfortunately for Column, the relative costs of being the first to depart from (1,1) do not suggest that Row—which would end up stuck with its seventh-best pay-off—would be more likely to move first. Certainly, in a real-world conflict there would be no reason for Column to choose this difficult problem in compulsion over the opportunity to have the possibility of success lie in its own actions by choosing strategy C or S.

But even if these more subtle distinctions are ignored, given the primary and secondary objectives of the game, Column should choose strategy C. The potential pay-off is equal to that of any other strategy, and the potential level of coercive action is lower than that of any other strategy.

Finally, as the final outcome matrix in Table 8 illustrates, strategy C is the domi-

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nant opening strategy for game D, the symmetrical nuclear game, with strategy S favored over strategy E.

The outcomes in both of the nuclear games are influenced by the presence of the brinkmanship option; however, actually taking this dangerous route can be avoided by farsighted players, provided that neither player begins the game with strategy E. To illustrate, consider the game matrix for the asymmetrical nuclear game in Table 7. If Column chooses an opening strategy of E and Row chooses S, resulting in an initial outcome of (2,7), Row's best farsighted move is to (1,1), to force Column to move to (7,4). In each of the nuclear games, moving from the initial outcome to (1,1) is always the best choice if the other player began the game with an E strategy.

Now suppose the initial outcome was the intersection of two S strategies, (8,3). If Column had the first opportunity to depart from the initial outcome, it could move to (2,7), but then Row would move to (1,1)—the brinkmanship option—and force Column to choose (7,4). This would allow Row to move the game to (9,5), where it would end. A farsighted Column player would be better-off moving directly from (8,3) to (9,5), consistent with the game's secondary objective—to achieve the pay-off at the lowest level of coercive bargaining. This would avoid the costs to its reputation for resolve that would be incurred in retreating from Row's act of brinkmanship.

What does this suggest about a real-world nuclear crisis? First, it highlights the importance of farsightedness, of looking ahead to avoid moves that will only add to the costs and risks of an inescapable outcome. As we have seen, opening the game with a more coercive strategy encourages the other player to respond in kind. Second, it illustrates the importance of understanding how each move affects the options available to the other side. It is important in the asymmetrical nuclear game, for example, that Column recognize that Row has no option other than a move to (1,1) should Column move from (8,3) to (2,7).

Table 8. Symmetrical Nuclear Game, Original Game Matrix and Final Outcome Matrix

	Column		
Row	C	S	E
	Game Matrix		
C	6,6	4,9	3,8
S	9,4	5,5	2,7
E	8,3	7,2	1,1
	Final Outcome Matrix		
C	6,6	5,5	8,3
S	5,5	5,5	8,3
E	3,8	3,8	3,8–8,3

Tit for Tat

By setting aside the REL prescription that parties dissatisfied with the outcome of the preceding crisis begin the next with a more coercive bargaining strategy, we found that the best initial bargaining strategy—for either party, in all four games—was to be more cooperative. It offered equal or higher pay-offs at lower levels of coercive behavior than either retaining the same bargaining strategy or moving to a more coercive strategy. Given the realpolitik design of the pay-off matrices, this is a surprising finding; but it is consistent with a number of studies suggesting the efficacy of the use of initially cooperative bargaining strategies in a wide range of conflict situations (Axelrod 1984; Esser and Komorita 1975; Osgood 1962), including interstate crises (Leng and Wheeler 1979).

The play of the games did not suggest that moving to a more coercive strategy was totally dysfunctional, rather that it was most effective as an option held in reserve as a potential response to a

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similar move by the other player. In this respect, effective play in the sequential crisis-learning games is close to a "tit-for-tat" or reciprocating strategy—open with cooperation, then respond in kind to the other side's moves. A tit-for-tat strategy, in fact, does quite well in these games. In 20 of the 24 possible instances, a player using a simple tit-for-tat strategy will achieve the same final outcome as that obtained by playing the best possible mix of farsighted strategies illustrated in Tables 5–8. And of the four exceptions, two end in compromise outcomes at CC, which possess their own long-term benefits. Since real-world crises are not games of complete information, in which the players possess the knowledge of each other's outcome preferences and the farsightedness presumed available to the players in the crisis-learning games, tit-for-tat would appear to be the most feasible strategy.[8]

Conclusions

The investigation of crisis-learning games has been a methodological departure from most game-theoretical examinations of crisis bargaining in that the foundation for the construction of the four crisis-learning games was laid in a data-based study of recurrent twentieth century crises (Leng 1983). Too often, formal modeling and data-based research have appeared to stand in opposition to each other. Yet, as the results of this study demonstrate, each has much to offer the other in the construction of more robust theories of interstate conflict behavior.

The set of hypotheses developed in the data-based study suggested the approach to modeling changes in the player's preferences from one crisis to the next, an area in which little previous work has been done by game theorists. By the same token, the formal modeling of the dynamics of the crisis-learning games offers new insights into the consequences of the initial strategy choices indicated by the findings in the data-based study, as well as providing a means to explore the prescriptive questions that they raised. The use of sequential games, in which myopic and nonmyopic outcomes are compared, illustrates the importance of evaluating the unfolding consequences of bargaining strategies over the course of an evolving crisis.

The substantive conclusions to be drawn from the crisis-learning games are limited to crises between states of relatively even capabilities and motivations, as well as by the simplifying constraints of the models, which assume farsighted, rational play with complete information. Nonetheless, they are very suggestive not only in relation to the empirical study of bargaining in recurrent crises (Leng 1983) but also in light of comparable findings in studies of conflict bargaining more generally.

One such finding is the critical importance of the choice of initial bargaining strategy. The realpolitik experiential learning hypotheses posited that with the exception of crises ending in mutually satisfactory compromises, at least one party would begin with a more coercive bargaining strategy than it employed in the previous crisis. The other party, equally determined to demonstrate its resolve, would respond in kind, setting in motion a sequence of increasingly coercive threats and counterthreats. There is a depressingly deterministic quality to the hypotheses—and the findings that support them—in the data-based study. Yet when that requirement was relaxed after the initial outcome in the crisis-learning games, the players still did not fare well, despite having the advantage of complete information over the full sequence of moves and countermoves.

This brought us to the prescriptive question of what is the most effective bargaining strategy in a recurrent crisis between evenly matched adversaries. The

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best *initial* strategy choice turns out to be radically different from that prescribed by REL reasoning. The most effective initial strategy is to be more cooperative, a finding that is consistent with studies of conflict bargaining by social psychologists (Esser and Komorita 1975), and in repeated plays of prisoner's dilemma games (Axelrod 1984). These studies more generally suggested the efficacy of an accommodative opening move followed by tit-for-tat responses on subsequent moves. Tit-for-tat or "reciprocating" bargaining strategies have also been found to be superior to escalating coercive bargaining strategies in real-world crises (Leng 1984; Leng and Wheeler 1979) and in extended deterrence (Huth and Russett 1987). That turned out to be the case in the crisis learning games as well.

What can we conclude from all of this? The relative effectiveness of one bargaining strategy or another is dependent on what is signaled to the other party regarding one's intentions and the effect that the move has on the options available to each player. Most studies of crisis bargaining, whether prescribing a strong demonstration of resolve (Ellsberg 1959; Jervis 1978; Snyder and Diesing 1977), or the importance of signaling a willingness to reciprocate cooperative moves (Leng and Wheeler 1979), have focused on the importance of the *intentions* signaled to the other player. The sequential learning games illustrate the importance of the second factor: the effect that each move has on the players' remaining *options*. Although the players in the crisis-learning games were possessed of unlimited farsightedness and complete information regarding the other player's preferences, certain outcomes were foreclosed once particular opening moves had been made, so that less desirable options became the only feasible moves. Beginning with a more cooperative move and then moving to tit for tat significantly reduced that danger.

The problem of foreclosing options is not considered in the 1983 study from which the games were constructed; nor does it receive more than passing consideration in other data-based studies of crisis behavior; yet it has been a critical factor in policy-making in real-world crises from the 1914 crisis to the U.S.-Soviet alert crisis of 1973. Thus, the immediate policy relevance of the games lies in their demonstration of the critical importance of having the foresight to avoid moves that can foreclose desirable options and force policymakers to consider acts of brinkmanship or war. As a theory-building exercise, the games suggest the need for a closer look at the role of this phenomenon in the evolution of real-world crises, as well as its implications for the relative effectiveness of different bargaining strategies, especially tit for tat.

Notes

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1. Brams and Wittman (1981) do not assign a final outcome to any departure unless one player has the opportunity to end the game at its most preferred pay-off. Only then is an outcome declared nonmyopically stable, and only if both players achieve their most-preferred pay-offs is it declared a nonmyopic equilibrium. Kilgour (1985) relaxes this condition by basing the concepts of nonmyopic stability and equilibrium on a comparison of each player's pay-off at the current outcome with that at the final outcome, assuming extended foresight on the part of the players.

2. One approach to a more detailed representation of the range of choices and outcomes might be through a presentation of the games in extensive form. This, however, runs the risk of depicting—in bargaining strategy choice—a degree of precision in making fine quantitative judgments along a continuum of cooperation and conflict that may be inconsistent with the necessarily cruder choices of real-world policymakers.

3. Two-by-two games are defined as *symmetric* if the outcome ranks are the same for both players along the main diagonal, with the ranks along the off-diagonal mirror images of each other. Here the

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term is used more loosely, as only the first condition is met.

4. The question of the circumstances under *which* party—if either—may move through (1,1) has been addressed by Zagare (1984) through adjustments in the theory of moves based on the strategic capabilities of the parties.

5. Brams and Hessel (1984) have introduced the notion of *threat power* to attempt to account for such real-world asymmetries in credibility in sequential games; however, the interesting aspect of the asymmetry in this instance is that it can occur as a result of the strategy choices in the course of the crisis, as opposed to being present at the outset.

6. In this last respect, the Cuban missile crisis does not fit the asymmetric game, which grants the brinkmanship option to the winner of the previous dispute in response to the loser's resort to a more coercive strategy.

7. The game is not a direct three-by-three analogue of the two-by-two game. Strategy E dominates C, but it does not dominate S.

8. The four exceptions: (1) With an initial outcome of (S,C) in the asymmetrical nonnuclear game, Column is better-off moving to strategy E on the first move rather than matching Row's strategy S; (2) the same situation obtains for Column in the asymmetrical nuclear game; (3) Row should depart from CC to SC on the first move in the asymmetrical nuclear game; (4) in the symmetrical nuclear game, with the initial outcome at CE, Row can do better by departing from tit for tat to move from EC to SC to achieve a final outcome of (9,5), rather than moving to CC for a (6,6) outcome.

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Russell J. Leng is F. C. Dirks Professor of Politics and Economics, Middlebury College, Middlebury, VT 05753.

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THE CORE AND THE STABILITY OF GROUP CHOICE IN SPATIAL VOTING GAMES

NORMAN SCHOFIELD
Washington University
BERNARD GROFMAN
University of California Irvine
SCOTT L. FELD
State University of New York Stony Brook

The core of a voting game is the set of undominated outcomes, that is, those that once in place cannot be overturned. For spatial voting games, a core is structurally stable if it remains in existence even if there are small perturbations in the location of voter ideal points. While for simple majority rule a core will exist in games with more than one dimension only under extremely restrictive symmetry conditions, we show that, for certain supramajorities, a core must exist. We also provide conditions under which it is possible to construct a structurally stable core. If there are only a few dimensions, our results demonstrate the stability properties of such frequently used rules as two-thirds and three-fourths. We further explore the implications of our results for the nature of political stability by looking at outcomes in experimental spatial voting games and at Belgian cabinet formation in the late 1970s.

Two of the most important questions in politics are the extent to which the outcome of collective-decision-making procedures can be expected to be (1) predictable and (2) stable. The game-theoretic notion of a core in principle provides important insight into both these questions. The *core* of a voting game is the set of undominated outcomes, that is, those that once in place, cannot be overturned. If games have a core, we expect that outcomes will be in the core and thus predictable. We consider those group-decision-making situations where the set of possible outcomes are points in (Euclidean) W-dimensional space. We shall refer to the phenomena we examine as *spatial voting games*. In this context the conditions under which a core can exist are now well understood. Even when a core exists, however, it may be *structurally unstable*, in the case that arbitrary small changes in preference are sufficient to render the core empty. We shall argue that the existence of structurally stable core outcomes is of fundamental importance in understanding political decision making.

For a simple-majority-rule voting game with no ties, the core, if one exists, is simply the majority winner, that is, that alternative which can receive a majority in paired contest against each and every other alternative. The practical difficulty

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is that under majority rule we expect in general there will be no core except under highly restrictive symmetry conditions (Davis, DeGroot, and Hinich 1972; Feld and Grofman 1987; Matthews 1980; McKelvey and Schofield 1987; McKelvey and Wendell 1976; Plott 1967; Schofield 1983a; Sloss 1973) or in the case that there is only one dimension (Black 1958). Subsequent work has deepened this pessimistic conclusion by showing that in general not only will there be no majority winner, but *all* alternatives will cycle with each other (Bell 1978; Cohen 1979; Cohen and Matthews 1980; McKelvey 1976, 1979; Schofield 1978a, 1978b, 1980, 1983a, 1985). Thus, by manipulating the agenda through restricting choices to some finite set to be voted upon in a specified order, *any* alternative can be made the group choice. This set of instability results is sometimes referred to as the "chaos theorems." (See the review in Riker 1980, 1982 for further discussion and alternative interpretations.)

Recent work has, however, provided grounds for a more optimistic view. There have been seven important lines of research. (Cf. an alternative typology in Grofman and Uhlaner 1985.)

The dominant line of research has been one that looks for *structure-induced equilibria.* Such equilibria occur because of imposed limitations on the set of alternatives that can be considered, for instance, a germaneness restriction, a closed rule, a mandatory final vote against the status quo, a budget constraint, a yes-no vote on an alternative proposed by an agenda setter, and so on (McCubbins and Schwartz 1985; Riker 1980; Romer and Rosenthal 1978; Shepsle 1979; Shepsle and Weingast 1981, 1984).

In another line of attack (Ferejohn, Fiorina, and Packel 1980; Ferejohn, McKelvey, and Packel 1984; Packel 1981), which has not yet been absorbed into the mainstream of the social choice literature, a group of scholars has shown that in spatial voting games there may be *probabilistic* convergence of outcomes to a small and well-defined area of the space centered around what has been called the *yolk.* Closely related is the work on agenda construction that demonstrates, among other things, that agendas that move toward the center of the yolk are much easier to construct than agendas away from the center (Feld and Grofman 1986; Feld, Grofman, and Miller 1985).

A third promising line is work on the uncovered set and various subsets thereof (Banks 1985; Banks and Bordes 1987; Feld and Grofman n.d.; Feld et al. n.d.; McKelvey 1986; Miller 1977, 1980, 1983; Miller, Grofman, and Feld 1985; Moulin 1984; Shepsle and Weingast 1984). The uncovered set can be thought of as a weakening of the concept of the core. The *uncovered set* is the set of points that are majority-preferred to all other alternatives either directly or at one remove; that is, if x is uncovered, then for all y either $x\ P\ y$ or there exists z such that $x\ P\ z$ and $z\ P\ y$.

A fourth new approach has been to focus on the Copeland winner (also known as the strong point). The *Copeland winner* is the alternative that is defeated by the fewest other alternatives (Copeland 1951; Glazer, Grofman, and Owen 1985; Grofman 1972; Grofman et al. 1987; Henriet 1984; Owen and Shapley 1985; Straffin 1980). The strong point, too, can be thought of as a weakening of the concept of core.

A fifth line of research has been to look at *von Neumann–Morgenstern externally stable solution sets,* which have the property that for any alternative outside the set, there exists an alternative in the set that beats it. Wuffle et al. (n.d.) show that the V-M externally stable solution of minimal area can play a role analogous to that of the core and that it is identical to the core if one exists.

Riker's (1982) work on a "liberal" concept of democracy as a referendum on the

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status quo may be thought of as constituting a separate sixth line of attack. It involves "a conceptualization of the notion of democracy so that the chaos of the McKelvey-Schofield results is irrelevant" (Riker, personal communication, 1986).

The seventh line of research is one that looks at supramajoritarian decision making and the minmax set (for definition see next section). Early work in this tradition includes Craven 1971; Ferejohn and Grether 1974; Kramer 1977; Packel 1981; Simpson 1969; and Slutsky 1979. It is this seventh line of research with which we will deal.

Recent work in this research tradition (Greenberg 1979; McKelvey and Schofield 1986, 1987; Schofield 1983b, 1984a, 1984c, 1985, 1986; and Strnad 1985) has, for spatial voting games, found conditions (a) under which a core will be guaranteed to exist, and (b) such that it is possible to construct a core that will be structurally stable. The research above states results not just for supramajoritarian games but also for the widest possible class of noncollegial voting rules (where a *noncollegial voting rule* means one in which no one voter or set of voters has sufficient power to be in all winning coalitions). These papers are *quite* technical; they appear (with one exception) in economics journals and provide (with one partial exception) no discussion of any potential or actual empirical applications.

Our aim is threefold: *first,* to present the most empirically relevant of these results in a nontechnical fashion for a political science audience, since it will turn out that these results really are not hard to understand, even though they are quite hard to prove in their most general form; *second,* to extend the results by reformulating key theorems in a way which makes their practical significance clearer; in particular, we look at the stability implications of two-thirds and three-fourths rules in two and three dimensions and at the stability characteristics of weighted voting and veto rules; *third,* to demonstrate the importance of these results for the understanding of actual group-decision processes by reanalyzing some recent experiments on spatial voting games (Fiorina and Plott 1978; Wilson and Herzberg 1984) and by offering an illustrative example of how we can model the stability of coalitions in multiparty coalition governments—looking at the 1978 Belgian political-party system.

Some of the most significant implications of the basic results we give are

1. When there are only two dimensions of choice, rules only marginally stronger (i.e., closer to unanimity) than a two-thirds rule will invariably give rise to a core, and rules that are only marginally stronger than simple majority may give rise to a structurally stable core if certain symmetry assumptions are met. Moreover, for two dimensions and few alternatives these conditions are not particularly stringent. In particular, in two dimensions, requiring only one voter above simple majority may make it possible to obtain a structurally stable core.
2. When there are only three dimensions of choice, rules only marginally stronger (i.e., closer to unanimity) than a three-fourths rule will invariably give rise to a core, and no rule that is less than two-thirds can give rise to a structurally stable core. Rules in between two-thirds and three-fourths may give rise to a structurally stable core if certain symmetry conditions are met.
3. Every voting situation in which there are one or more players with veto power has a core.
4. The core will always exist if the only feasible coalitions are those that are

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connected (Axelrod 1970) with respect to some given single dimension.

Basic Results about the Minmax Set and Structurally Stable Voting Rules

Let n be the number of voters. Let q be the number of votes needed to replace the status quo with a new alternative. We shall refer to such decision rules as *quorum rules* or *q-rules*. For n odd, if $q = (n + 1)/2$ we have simply majority rule. If $q > (n + 1)/2$, that is, if decision making is supramajoritarian, then we may obtain a core. (Of course we must always obtain a core if $q = n$.)

To examine spatial voting games that are q-rules, we need to introduce a few concepts. We suppose that the set of outcomes is a subset of Euclidean space of dimension W. We generally assume the set is convex and compact (i.e., closed and bounded) unless we state otherwise. We also assume that individual preference is *convex* (i.e., for each alternative the set of preferred alternatives is a convex set).[1] To further simplify our exposition, we assume that the game is what is called a tournament; that is, there are no ties. Dealing with ties is, however, straightforward. For a given dimension W and society size n, we can find the *smallest* integer q such that any q-rule, with $q' \geqslant q$, must have a nonempty core. The core for the q-rule so defined is called the *minmax set* in dimension W (Simpson 1969; Kramer 1977).

Kramer (1977) showed that a sequential two-candidate spatial voting game, in which candidates sought to maximize their plurality, converged to outcomes in the minmax set. Let $q^* = q/n$; that is, since q is the minimum number of players in a winning coalition then q^* is the size of the minimal winning majority expressed as a proportion. Kramer (1977, 328, n. 4) conjectured that the smallest q^* that guarantees (i.e., is *sufficient* for) the existence of a core was given by $W/(W + 1)$, where W is the dimensionality of the space. We shall refer to the minimum q^* which guarantees the existence of a core as the *Kramer number*, and denote it q_k^*. Kramer's conjecture was proved by Greenberg (1979) and generalized further by Schofield (1983b, 1984a) and independently by Strnad (1985).

If a q-rule satisfies $q/n < q_k^*$, it is possible to show that there is a configuration of convex voter preferences such that the core is empty. However, it may also be possible to find a configuration of convex voter preferences for which a core *does* exist and is, moreover, structurally stable.

Results in Schofield (1986) can be used to prove a result closely related to Kramer's conjecture: for large electorates the minimum q^* for which there exists a configuration of voter preferences resulting in a structurally stable core is given by $(W - 1)/W$. We shall call this the *Schofield number* q^*_s. We shall also provide an exact expression that relates q and q^*_s to n and W.

In much of the literature, results are stated in terms of the lowest dimension in which instability occurs (the *instability dimension*). Here, we shall focus on finding what q-rules are sufficient for stability, or give rise to the possibility of a structurally stable core, when there are only a few dimensions. We believe such results are of considerable practical relevance, since much real-world decision making takes place with only a very limited set of issue dimensions under simultaneous consideration.

In this section we state in informal terms the key theorems and some implications derived from them.

THEOREM 1. *For any given n, the minimum value of q sufficient to guarantee the existence of a core in a q-rule game*

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is given by the smallest integer q such that

$$q > \frac{nW}{W+1} \tag{1}$$

Greenberg (1979) shows that for supramajoritarian decision rules, the minimum dimensionality sufficient to guarantee a core is given by $W < q/(n-q)$. Theorem 1 follows after some elementary algebra.

Greenberg (1979) and Schofield (1984b) have also shown that if W is an integer and satisfies $W \geqslant q(n-q)$ then it is possible to find a set of preferences such that the core is empty. Thus, if $q \leqslant nW/(W+1)$, the existence of a core cannot be guaranteed.

It is useful to express Theorem 1 in terms of proportions rather than absolute numbers. As before, let $q^* = q/n$.

COROLLARY 1 TO THEOREM 1. *For any given n, the Kramer number, q_k^*, i.e., the minimum special majority, q^*, sufficient to guarantee the existence of a core, is given by the smallest q_k^* which is a multiple of 1/n such that*

$$q_k{}^* > \frac{W}{W+1} \tag{2}$$

Proof. Substitute $q^* = q/n$ in Relationship 1. QED

COROLLARY 2 TO THEOREM 1. *For $W = 1$, the one-dimensional case, for any value of n, there exists a majority-rule core.*

Proof. Substituting $W = 1$ in Relationship 1 we obtain $n/2$. The smallest integer greater than $n/2$ is $(n+2)/2$ for n even, and $(n+1)/2$ for n odd. QED

This corollary is familiar as Black's famous (1958) median-voter theorem, since, in one dimension, for simple-majority rule, the ideal point of the median voter is in the core.

COROLLARY 3 TO THEOREM 1. *For $W = 2$, the two-dimensional case, for any value of n, if $q^* > 2/3$ then a core is guaranteed.*

Proof. Substitute $W = 2$ in Relationship 2. QED

Analogously, for 3 dimensions, a core is guaranteed if $q^* > 3/4$, and so on.

Theorem 1 provides conditions sufficient to guarantee the existence of a core for q-rules.

Even though a core cannot be *guaranteed* if $q \leqslant nW/(W+1)$, for a high enough q, a structurally stable core can be constructed by appropriately configuring voter preferences, as the following theorem proves:

THEOREM 2. *For any given n, any q-rule satisfying*

$$q \geqslant \frac{2 + n(W-1)}{W} \tag{3}$$

a structurally stable core can be constructed.

Schofield (1984b, 1986) shows that for supramajoritarian decision rules, a structurally stable core can be constructed for some configuration of preferences if

$$W \leqslant \frac{n-2}{n-q}. \tag{4}$$

Theorem 2 follows after some simple algebra.

It is useful to express Theorem 2 in terms of proportions rather than absolute numbers. Let $q^* = q/n$ as before.

COROLLARY 1 TO THEOREM 2. *For any given n, the Schofield number, q_s^*, i.e., the special majority, q^*, sufficiently large to permit the existence of a structurally stable core is given by the smallest q_s^* which is a multiple of 1/n such that*

$$q_s^* \geqslant \frac{(W-1)n+2}{Wn} = \frac{W-1}{W} + \frac{2}{Wn}$$

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Proof. This follows directly from Relationship 3 by substituting $q = q^*n$. However, we must be careful to confine ourselves to q^* which are multiples of $1/n$. QED

COROLLARY 2 TO THEOREM 2. *For $W \geqslant 2$, then as n gets large, the Schofield number, q_s^*, is given by*

$$\lim_{n\to\infty} q_s^* = \frac{W-1}{W}$$

Proof. Follows directly from Relationship 2. QED

COROLLARY 3 TO THEOREM 2. *For $W = 2$, for given n, the smallest integer q sufficiently large to permit the existence of a structurally stable core is given by $n/2 + 1$ for n even and $(n + 1)/2 + 1$ for n odd.*

Proof. If n is even, Relationship 4 gives $W \leqslant (n-2)/(n-q)$ or $q \geqslant n - n/2 + 1$ in the case $W = 2$. Hence $q \geqslant n/2 + 1$. If n is odd, the largest integer greater than $(n + 2)/2$ is simply $(n + 1)/2 + 1 = (n + 3)/2$. QED

Corollary 3 to Theorem 2 shows that it may be possible to generate a structurally stable core in two dimensions with q only one more than simple majority.

More generally, Corollary 2 to Theorem 2 shows that for a very large electorate, it is always possible to construct a structurally stable core as long as a sufficiently high quorum rule $(W - 1)/W$ is chosen. For example, for $W = 3$ we can always find a way to construct a structurally stable core if $q^* > 2/3$ (i.e., we can always find a configuration of preferences in three-dimensions for which such a core will exist). It may be possible to find a structurally stable core for lower quorum rules, as the following necessary condition indicates.

THEOREM 3. *For any given n, then for a structurally stable core to exist it is necessary that*

$$q \geqslant \frac{W + n - 1}{2} \tag{5}$$

McKelvey and Schofield (1986) have shown that if $W \geqslant 2q - n + 2$, then a structurally stable core can never exist on a compact set of alternatives. Thus a necessary condition for a structurally stable core is $W \leqslant 2q - n + 1$. Rearranging gives the desired result.

For $W = 2$, Relationship 5 is simply $q \geqslant (n + 1)/2$; that is, in two dimensions with n odd a structurally stable core can occur for simple-majority rule. However, the techniques used by McKelvey and Schofield (1986) show that it must then lie on the boundary of the set of alternatives (see also McCubbins and Schwartz 1985). Moreover, McKelvey and Schofield (1986) show that if the set of alternatives has an empty boundary, then a structurally stable majority-rule core in two dimensions with n odd is impossible. More generally, if the set of alternatives is open, then a necessary condition for a structurally stable core is that $q \geqslant (W + n)/2$.

Define $[(W + n)/2]$ to be the smallest integer greater than or equal to $(W + n)/2$ and denote $q_m^* = 1/n[(W + n)/2]$, the *McKelvey* number.

Writing the Kramer, Schofield, and McKelvey numbers as q_k^*, q_s^* and q_m^* note that $q_k^* \geqslant q_s^* > q_m^*$ for any dimension and electorate size.

As yet it is not known whether $q \geqslant (W + n)/2$ is sufficient in general to guarantee the possibility of a structurally stable core. In other words there are decision rules that lie between q_s^* and q_m^* for which we are not sure whether a structurally stable core configuration of voter preferences can be constructed.

Note that if a structurally stable core is possible, then the probability (P) of obtaining a core must be nonzero when preferences are randomly distributed. Thus we can rephrase the previous results as

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Table 1. Supramajoritarian Decision Rules That Guarantee a Core and Those That Make Possible a Structurally Stable Core

Number of Votes	W (Dimension) 1		2		3		4			∞	
	q_k^*	q_s^*	q_k^*	q_s^*	q_k^*	q_s^*	q_k^*	q_s^*	. . .	q_k^*	q_s^*
3	$\frac{2}{3}$	$\frac{2}{3}$	$\frac{3}{3}$	$\frac{2}{3}$	$\frac{3}{3}$	$\frac{3}{3}$	$\frac{3}{3}$	$\frac{3}{3}$			
4	$\frac{3}{4}$	$\frac{3}{4}$	$\frac{3}{4}$	$\frac{3}{4}$	$\frac{4}{4}$	$\frac{4}{4}$	$\frac{4}{4}$	$\frac{4}{4}$			
5	$\frac{3}{5}$	$\frac{3}{5}$	$\frac{4}{5}$	$\frac{4}{5}$	$\frac{4}{5}$	$\frac{4}{5}$	$\frac{5}{5}$	$\frac{4}{5}$			
7	$\frac{4}{7}$	$\frac{4}{7}$	$\frac{5}{7}$	$\frac{5}{7}$	$\frac{6}{7}$	$\frac{6}{7}$	$\frac{6}{7}$	$\frac{6}{7}$			
9	$\frac{5}{9}$	$\frac{5}{9}$	$\frac{7}{9}$	$\frac{6}{9}$	$\frac{7}{9}$	$\frac{7}{9}$	$\frac{8}{9}$	$\frac{8}{9}$			
27	$\frac{14}{27}$	$\frac{14}{27}$	$\frac{19}{27}$	$\frac{15}{27}$	$\frac{21}{27}$	$\frac{19}{27}$	$\frac{22}{27}$	$\frac{21}{27}$			
43	$\frac{22}{43}$	$\frac{22}{43}$	$\frac{29}{43}$	$\frac{23}{43}$	$\frac{33}{43}$	$\frac{30}{43}$	$\frac{35}{43}$	$\frac{33}{43}$			
101	$\frac{51}{101}$	$\frac{51}{101}$	$\frac{68}{101}$	$\frac{52}{101}$	$\frac{76}{101}$	$\frac{68}{101}$	$\frac{81}{101}$	$\frac{77}{101}$			
501	$\frac{251}{501}$	$\frac{251}{501}$	$\frac{335}{501}$	$\frac{252}{501}$	$\frac{376}{501}$	$\frac{335}{501}$	$\frac{401}{501}$	$\frac{377}{501}$			
∞	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{2}{3}$	$\frac{1}{2}$	$\frac{3}{4}$	$\frac{2}{3}$	$\frac{4}{5}$	$\frac{3}{4}$	. . .	1	1

Note: q_k^* is the Kramer number, the decision rule that guarantees that a core will exist; q_s^* is the Schofield number, the decision rule such that a structurally stable core is possible for some configuration of voter preferences.

follows: P(core / $q^* \geqslant q_k^*$) = 1; P(core / $q^* \geqslant q_s^*$) > 0; and P(core / $q^* < q_m^*$) = 0.

In Table 1, for given values of n and W, we show the Kramer number (i.e., the values of q^* sufficient for a core) and the Schofield number (i.e., the values of q^* for which it is always possible to find a set of preferences with a structurally stable core). For example, if there are nine voters, then in two dimensions a core is guaranteed if $q = 7$ (i.e., using a seven-ninths rule), while a structurally stable core is possible if $q = 6$ (i.e., a two-thirds rule). For the case of four voters in two dimensions, a three-fourths rule guarantees a core. In like manner we may read out results from Table 1 for any given values of W, n, and q. Note that, as indicated above, $q_k^* \geqslant q_s^*$ for all W and n.

We shall provide a graphical illustration of a game without a core, a game with a structurally unstable core, and a game with a core that is structurally stable. First, however, we look at some additional results for collective-decision rules in which not all players are weighted equally.

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Weighted Voting Games

Most political systems do not use majority rule for all decisions; not only do they require extraordinary majorities (q^* rules) for certain kinds of decisions (e.g., in situations ranging from constitutional changes to confirmation of ambassadorial appointments), but they also make use of hierarchies of committees or constituencies and veto and override procedures. Results similar to those in the theorems given above can be obtained for these more general kinds of voting rules (for work in this direction, see Schofield n.d.[a]). We provide below some important results for the class of weighted voting games.

In a *weighted voting game* each player, i, has a weight, p_i, and there is a quorum rule, q^*, such that a motion passes only if the sum of the weights of the players who support it is at least q^*. For convenience, we let $\Sigma p_i = 1$. For example, we may have a five-voter game where weights are 4/11, 3/11, 2/11, 1/11, and 1/11, respectively, with a simple majority, six-elevenths, needed for passage, or we might increase the number of votes needed for passage, say, to eight-elevenths. The first game is commonly denoted (6;4,3,2,1,1), and the second (8;4,3,2,1,1). In the first game, the extension of Greenberg's Theorem (Greenberg 1979) by Schofield (1985) shows that a core can only be guaranteed in one dimension but that a structurally stable core exists in two dimensions. (See Theorem 5 below.) In the second game, the player with a 4/11 weight belongs to every winning coalition, and so that player's most preferred point is the core (see Theorem 4 below).

Weighted voting games are a much more general concept than they might first appear. Almost all multitiered representation systems with veto power or veto power with override that are in common use can be reexpressed as weighted voting games (Brams 1975; Straffin 1980). For example, in the UN Security Council, the Big Five (U.S., USSR, Great Britain, France, China) have only one vote apiece but they also have veto power (a negative vote cast by any of them defeats any motion); the 10 other members of the Security Council have 1 vote, and, absent a veto, 9 of 15 votes are needed for passage. This voting situation can be reexpressed in weighted-voting terms as (39;7,7,7,7,7,1,1,1,1,1,1,1,1,1,1). Note that we now have a thirty-nine forty-fifths q^* rule (Lucas 1976). To see that this weighted voting game is equivalent to the five-power veto, merely note that a bill can pass if and only if at least 9 countries vote for it, including all 5 of the big powers. Call a game with a veto player a *collegial game*.

In illustrating the existence of a core, it is often convenient to assume that each voter has preferences that can be represented in terms of the distance from the voter's ideal point. Call such a situation a *Euclidean voting game*. In general, when the core exists, it will be the *convex hull* of some set of voters' ideal points (i.e., the smallest convex set containing the particular points).

There is a simple result for weighted voting games that are collegial.

THEOREM 4. *All collegial weighted voting games have a core. If voter preferences are Euclidean, then the core contains the convex hull of the vetoers' ideal points. (In particular, games with a single veto player have the core located at that player's ideal point.)*

This result follows straightforwardly from Schofield (1985, Corollary 4.3.8).

For noncollegial weighted voting rules, a quite general result can be stated. First, a definition is essential.

DEFINITION. *The Nakamura number, N is the cardinality of the smallest set of winning coalitions with the property*

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that the intersection of its members is empty (Nakamura 1979; Schofield 1984a).

Since this definition is not at all obvious, an example will help. Consider the five-voter game (6;4,3,2,1,1). Label the players A, B, C, D, and E in that order. The minimal winning coalitions are AB, AC, ADE, BCD, and BCE. Any two minimal winning coalitions have at least one member in common, that is, their intersection is nonempty. Such games are said to be *proper*. If, however, we consider the coalitions {A,B}, {A,C} and {B,C,D}, these three have an empty intersection. With three minimal winning coalitions we can get a nonempty intersection, but with two coalitions the intersection is nonempty. Hence the Nakamura number for this example is 3.

THEOREM 5. *Let N be the Nakamura number. If $N \geqslant W + 2$ then the core is nonempty.*

For a proof see Schofield 1984a.

To see how Theorem 5 generalizes Theorem 1, consider a q-rule with $n = 5$ and $q = 4$. The winning coalitions {ABCD}, {ABCE}, {ABDE}, {ACDE}, and {BCDE} do not intersect, and so $N = 5$. By Theorem 5 a core is guaranteed only if $W \leqslant 3$. Greenberg's result (Theorem 1) requires that $W < 4/(5 - 4)$; that is $W \leqslant 3$.

For weighted voting games (and other more complex games), the Nakamura number tells us in what dimensions it will be possible to construct a core. The higher the Nakamura number, the higher the dimensionality in which outcomes of the game are predictable. For example, if $N = 3$, then a core can exist in only one dimension. By convention, if a game has a veto player, then we shall define the Nakamura number to be infinity. Hence, if a game has a veto player, by Theorem 5 it always has a core regardless of the dimensionality of the space; that is, Theorem 4 can be taken to be a direct corollary of Theorem 5.

One important way in which a core can be created in weighted voting games is if certain coalitions are prohibited. This can have the effect at making the game collegial. For example, take the game (6;4,3,2,1,1) that we previously mentioned. If we now require that the only coalitions that can form are those that are connected along the *ABCDE* dimension (i.e., if, say, A and C are in a coalition, then B, who is between them, must also be in that coalition) then the only minimal winning (connected) coalitions are {A,B} and {B,C,D} (see Axelrod 1970 and Grofman 1982). Hence the game is now collegial. Let us take a further example (9;4,3,2,1,1,1,1): if we imposed a connectedness requirement, then the only feasible coalitions would be {A,B,C} and {B,C,D,E,F,G} since most of the (minimal) winning coalitions are not connected. Again, we would have a collegial voting game and thus a core.

COROLLARY 1 TO THEOREM 5. *If the only feasible winning coalitions are those that are connected in some given dimension and the game is proper (i.e., the complement of any winning coalition is losing), then there always exists a core.*

Outline of Proof. Since the game is proper, the Nakamura number is at least 3, so by Theorem 5 there always exists a core in one dimension (as in Black 1958). But then there exists at least one player who belongs to every connected feasible winning coalition. Hence the game is collegial, and by Theorem 4 the core is nonempty. QED

Many examples where certain coalitions are "ruled out" occur in real life; for instance, the juxtaposition of ideological extremes in the same coalition is quite unlikely and certain parties (e.g., the

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Communists and the far right in many European countries) are not regarded as possible coalition partners. Also, some parties choose not to be part of the governing coalition. Reducing the number of relevant players in the set of feasible winning coalitions increases the likelihood that winning coalitions will intersect. That is to say, the existence of parties who choose to remain outside the government may increase the size of the Nakamura number and thus promote stability. More generally, the fewer the parties who enter the coalition game, the more likely the game is to be collegial. This line of argument indicates that a multiplicity of parties intent on entering government can make it less likely that there will be a core —and the absence of a core makes for political instability in the form of an absence of enforceable political consensus.

Stability may also occur if groups in a weighted-voting-rule setting (of which simple-majority rule is a special case) operate under a de facto rule that is more than the de jure rule. Then, such a consensus-oriented norm (even though slight) may allow a core to exist. For example, while (8;4,3,2,1,1,1,1) need not have a core in two dimensions, (9;4,3,2,1,1,1,1) does, as we shall show below. Such pressure toward larger than minimal majority coalitions may occur if certain decisions, such as veto override, require more than simple majority or if coalitional allegiances are uncertain or simply if social norms exist that lead voters to try to seek consensus in order to avoid conflict. More generally, Schofield (1986) has conjectured that the imposition of multiple tiers or other forms of complex (but noncollegial) weighted voting rules will make likely the existence of a structurally stable core even in issue spaces of relatively high dimensions.

When there are several key actors who are found in almost all minimal winning coalitions, the Nakamura number will be relatively high, so we will be able to construct a core in two or perhaps even three or more dimensions. We provide an example of such a game: (9;4,3,2,1,1,1,1) with players A, B, C, D, E, F, G. Here the minimal winning coalitions are ABC, ABDE, ABDF, ABDG, ABEF, ABEG, ABFG, ACDEF, ACDEG, ACDFG, ACEFG, and BCDEFG. Any two or any three minimal winning coalitions have a nonempty intersection. But, for example, {A,B,C}, {A,B,D,E}, {A,C,D,E,F} and {B,C,D,E,E,G} have an empty intersection. Hence the Nakamura number is 4. Since $4 - 2 = 2$, there must exist a nonempty core in two dimensions.

One further observation worth emphasizing is that, for Euclidean preferences, in the highest dimension that a structurally stable core for a particular game is possible, the core may be at the preferred point of one of the players, that is, the minmax set will be a point. In general that player will be the most powerful player in the game. We now discuss an example to illustrate this possibility and also provide an example of a weighted voting game that occurred in Belgium in 1978 for which no majority-rule core exists. We then show how a structurally stable core for this game can be created in two dimensions.

Applications of Basic Results to Experimental Games and Cabinet Formation

Our results suggest that at least when there are only a few dimensions (e.g., $W = 2$ or 3), social-decision processes are apt to result in stable choice within a delimited area of the space if groups use a supramajoritarian procedure or a weighted voting rule. For example, in each of three games with equally weighted players where there is no majority-rule core—a game in two dimensions with five players used in experiments by Wilson and Herzberg (1984, Fig. 3), a game in

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two dimensions with seven players considered in Kramer (1977, 321), and another two-dimensional five-voter game, Game 3 of the experiments by Fiorina and Plott (1978, 579, Fig. 3)—the minmax set appears to have useful predictive powers.[2] Also, in all three games the minmax set is relatively small.[3] In each game we find that a rule of one more than simple majority gives rise to a core, since 4/5 > 2/3 and 5/7 > 2/3 (see Corollary 2 to Theorem 1).

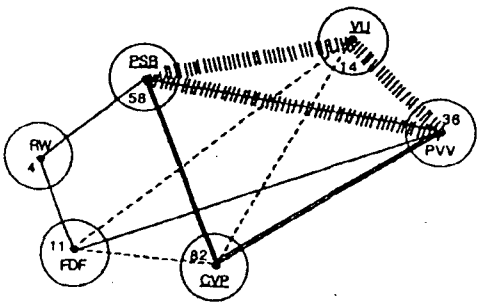
We now wish briefly to consider the results of the 1978 Belgian general election. We show in Figure 1 a two-dimensional spatial map of the Belgian party system in 1978 based on data prepared by Hearl under the auspices of the European Party Manifestos Project directed by Ian Budge (Budge, Robertson and Hearl 1987).

The core cannot be guaranteed in two-space in that election since three of the minimal coalitions, namely {CVP,PSB}, {CVP,PVV}, and {VU,PVV,PSB} have an empty intersection. Hence the Nakamura number is 3, and by Theorem 5, the core is guaranteed only in one dimension. For purposes of exposition we shall assume that each party has Euclidean preferences based on its ideal point. In Figure 1 we associate to each winning coalition the convex set generated from its members' ideal points. For the core to be nonempty, the five relevant sets must intersect, which they clearly do not.

Another way to see this is to use the notion of "median lines." Consider the line through the VU and CVP positions. The parties on and to the "left" of this line comprise a winning coalition with 169 seats, while the parties on and to the right of the line also comprise a winning coalition with 132 seats. Such a line is a median line, and for the core to be nonempty it must lie on the intersection of all median lines. However, the median lines VU–CVP, PVV–FDF and CVP–PSB do not intersect. (For a nontechnical exposition of the geometry, see Feld and Grofman 1987. For an extension of this analysis to a situation with non-Euclidean preferences, see McKelvey and Schofield 1987).

However, if we move the CVP to be colinear with the PVV and the PSB, as shown in Figure 2, then there is a core at the location of the CVP. Note, however, that if the CVP position is moved slightly, then the core becomes empty since once again the set of median lines does not intersect at a single point. To see this, note that the line connecting the PSB and PVV remains a median line even though it

Figure 1. Belgian Political-Party Space and Seats Held in 1978



PSB Socialist party
VU Volksunie
FDF Francophone Front
RW Rassemblement Wallon
PVV Liberals
CVP Christelijke Volksparti

Minimal Winning Coalitions

CVP, PSB — 140 seats
CVP, PVV — 118 seats
RW, FDF, PVV, PSB — 110 seats
PSB, VU, PVV — 108 seats
FDF, CVP, VU — 107 seats

Source: Adapted from Derek Hearl, Belgium 1946–1981, in *Ideology, Strategy, and Party Change*, ed. Ian Budge, David Robertson, and Hearl (Cambridge: Cambridge University Press, 1987).

Note: Underlined parties in the figure are members of the cabinet (PSB and CVP). Independents (I, 3 seats) and Communists (KPB, 4 seats) are not shown. $N = 212$; 107 seats are needed for majority.

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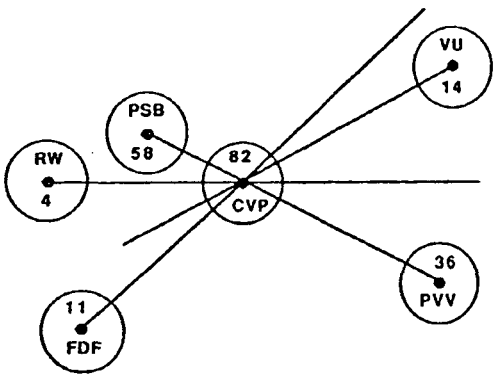
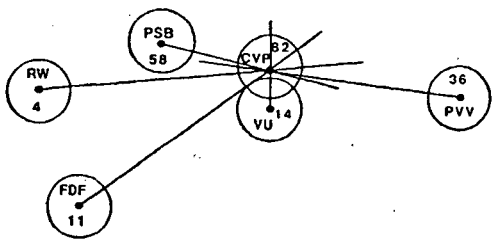
Figure 2. Hypothetical Transformation of Figure 1 to Create a Structurally Unstable Majority-Rule Core



no longer passes through CVP. Thus the core in Figure 2 is structurally unstable. However, we can move party locations so as to create a core. For example, if both the CVP and VU positions are changed, as in Figure 3, then there exists a structurally stable core at the preferred point of the CVP, because now the line between PSB and PVV is not a median line and all median lines pass through CVP.

There is some evidence that the parties tend to change position in this policy space, as though "hunting the core." We can see a possible reason for this phenomenon. In a situation such as represented in Figure 1, no matter which coalition were to form, there always exists another majority group that stands to gain by bringing down the government. One would therefore expect governments to be quite short-lived. Indeed, in Belgium in the period since 1971, average coalition-government duration is 16 months (Schofield n.d.[b]). However, as Figures 2 and 3 illustrate, if the party positions were different, then a core could exist. Suppose the core were nonempty but structurally unstable, as in Figure 2. No winning coalition has in fact an incentive to bring down the government, but the smallest error in judgment or perception would lead parties to the belief that there were such an incentive. Only for the structurally stable core situation would one expect relatively long-lived governments.

Figure 3. Hypothetical Transformation of Figure 1 to Create a Structurally Stable Core



Short-lived government does impose a cost on parties. If parties are willing to make a trade-off between their preferred policy positions and the advantages accruing from membership of government, then they may be aware of the fact that changing policy position somewhat can in fact lead to a core situation. Moreover, if one party makes the correct type of policy movement, then its preferred point would indeed be at the structurally stable core. In that case the party would know that it would belong to the government coalition and would also expect that the government coalition would be relatively long-lived. Thus the game has a high strategic context as parties become aware of the advantages of finding the structurally stable core.

Instead of moving party locations, another way to obtain a stable core is to impose a supramajoritarian rule on a coalition game. In particular, for Belgium in 1978, if we require 125 or so votes to have a "comfortable" (but not unnecessarily large) majority, then the only minimal winning coalitions (with Communists and independents excluded) are those that include the CVP.

A third way we can create a core is by treating one of the dimensions as more

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important, and positing that all coalitions will be connected with respect to that dimension. In the example of Figure 1, if we collapse to the x-axis (i.e., look at party projections on that axis), then CVP is in the core.

A point worth noting from the Belgian case that we just examined is that the CVP is the only party whose preferred point can be a structurally stable core *point*. The next largest party, the PSB, with just under 30% of the seats, is too small to be in a structurally stable core position. (See Schofield 1986 for a fuller discussion of this example.) It is evident that this feature endows the CVP with far greater bargaining power than any other party.

To adapt Daalder's (1971) terminology, it is reasonable to identify the Belgian political system as *unipolar*. The same may be said of the Netherlands and Italy, as well as certain other countries. In some countries, on the other hand, the dimensionality of the policy space is such that *two* quite different structurally stable cores may occur, each one at the preferred position of one of two parties. Such a system could be called *bipolar*. This notion could be carried further to identify *multipolar* systems as those where three or more parties may occupy the structurally stable core position. All such parties could be regarded as being of equal power, irrespective of the differences in the actual number of seats they control. Note that this suggestion, if valid, gives a very precise method of formalizing the qualitative analysis of political party systems carried out by Daalder (1971), Sartori (1966) and others. In other words, by specifying how many of the parties in a given system could be core points, we can provide a nonarbitrary classification for party systems as unipolar, bipolar, tripolar, and so on (see Schofield n.d.[a]).

The 1978 Belgium example is intended to be purely illustrative. We hope in further work to pursue the question of adequately modeling the dynamics of party evolution over time (in Belgium and elsewhere) and to trace the link between the existence of core points in party spatial arrays and features of political life such as cabinet stability, voter volatility, and sharpness of ideological conflict.

Discussion

We believe that understanding the conditions under which group choice can be expected to be stable is important. Policy cannot be intelligibly formulated or intelligibly implemented in an environment where decisions can always be revoked and no outcome is final. The results we have presented here suggest one important reason why, in the real world, stable outcomes occur. Outcomes in the minmax set offer the minimum consensus needed to achieve stability. Once an outcome in the minmax set has been picked, it is unlikely to be altered, especially if the core is structurally stable.[4]

Moreover, we believe that outcomes in the minmax set occur even in situations where the *formal* voting rule is one that is insufficient to give rise to a core. The reason for that view is quite simple. In real groups there is a reluctance to permit "bare"-majority decision making. Rather, consensus is sought (Zablocki, 1971, 155–58). Also in the political-party context, bare-minimum majorities are unsafe, because they are too vulnerable to blackmail through threats of defection. Thus, the de facto rule in group decision making is apt to be larger than the de jure rule.[5] This may also help explain why, in experiments on spatial voting games, outcomes may tend to be "tightly clustered" (Fiorina and Plott 1978, 590).

For decision processes where all voters have the same weight, we have shown that stability can occur with relative ease when the dimensionality of the space is low. In this case decision rules very near to bare majority may give rise to a struc-

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turally stable core, and a two-thirds-plus rule (in the case of $W = 2$), or a three-fourths-plus rule (in the case of $W = 3$) will *guarantee* a core. We believe this finding can account in part for the popularity of such supramajoritarian rules. We have also shown that, even if the dimensionality of the space is relatively high, then (as with the UN Security Council example) veto rules or other special features may create either collegial voting rules or weighted voting games whose spatial array is such as to give rise to a core. These results significantly ameliorate the pessimism that seemed warranted by the generic instability results for simple-majority rule.

Finally, we have suggested ways in which the structural features of voting games can be used to provide a nonarbitrary classification of party systems as unipolar, bipolar, and so on. Also, we have identified hypotheses about electoral dynamics and cabinet longevity that are based on the spatial array and relative strength of political parties and the conditions of a structurally stable core, which suggests a new and promising line of research on a topic that has been much studied but is still not well understood (Grofman n.d.; Schofield n.d.[a]).

Notes

A portion of this research was conducted while Schofield was the Sherman Fairchild Scholar in the Division of Humanities and Social Sciences, California Institute of Technology. The work has benefitted from conversations with Charles Plott and earlier collaborative research with Richard McKelvey. We are indebted to the staff at the Word Processing Center, School of Social Sciences, University of California, Irvine for typing earlier drafts of this manuscript; to Cheryl Larsson for preparation of the figures; and to Dorothy Gormick for bibliographic assistance. This research was partially supported by NSF Grant no. SES 84-18295 to Schofield, and no. SES 85-06376, Program in Decision and Management Science, to Grofman. It was prepared for presentation at the International Conference on European Cabinet Coalitions, European University Institute, Fiesole, Italy, 1987. Participation at the conference by the first two authors was funded by NSF Grant no. SES 85-21151, Political Science Program.

1. If the convexity and compactness assumptions are dropped, then a "local core" is obtained. See Kramer and Klevorick 1974 and Schofield 1985 for the technical details. That readers grasp the exact nature of the compactness and convexity assumptions is not at all critical for understanding what follows. Readers may act as if all results refer to the familiar Cartesian coordinate space, thereby treating distance between alternatives as if that distance satisfied those properties it would satisfy in ordinary life.

2. Wilson and Herzberg (1984) in their experiments on five-voter spatial games used both simple-majority rule and a four-fifths rule. In both cases the majority of the outcomes was either within the minmax set (10 of 18 cases for the four-fifths rule, 8 of 18 for the three-fifths rule) or very near to it (an additional 6 of 18 cases for the four-fifths rule and an additional 7 of 18 cases for the three-fifths rule). Moreover, in the Wilson and Herzberg (1984) experiments, of the few outcomes that were not in or near the minmax set, half occurred as outcomes that were imposed when time ran out.

In the Fiorina and Plott (1978) experiment of Game 3, while only 3 of 15 observations fall directly within the minmax set, if we enlarge the bounds of the minmax set slightly, we can form a polygon in which 13 of the 15 observations fall. Even the remaining two observations are not so far away from the minmax set. Indeed, the mean outcome (45,62) in this voting game is within the minmax set (albeit barely). Because Fiorina and Plott (1978) counted success only when an outcome was inside the minmax set, they were not impressed with the fit of the minmax set to their data (cf. Ferejohn and Fiorina 1975). Although they concluded that "the minmax set does best among the models considered" (Fiorina and Plott 1978, 592), they then went on to say that was "only because of very weak competitors." However, there were 14 of these so-called "weak" competitors and we believe that *visually*—the most powerful statistical test known—the Fiorina and Plott experiments show the minmax set to fit the data quite well (see Fiorina and Plott 1978, 589, Fig. 8).

Also, we should note that other explanations of the location of outcomes in the Fiorina and Plott (1978) experiments on games without a core have been offered by Margolis (1982) and Grofman et al. (1987). A variety of models to predict expected outcomes in such games have been proposed. See Fiorina and Plott (1978) and Straffin and Grofman (1984) for relatively nontechnical reviews.

Wilson and Herzberg (1984) also ran experiments on the same five-voter two-dimensional game that we discussed, but now with one player (Player 2)

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given a veto. Though Wilson and Herzberg (1984) do not treat the game this way, it can be reexpressed as the weighted voting game (5;3,1,1,1,1). This game is collegial (i.e., there is a veto player who is in all minimal winning coalitions). Giving Player 2 a veto power shifts the location of the core away from the center of the space and to the ideal point of that player. In the Wilson and Herzberg (1984) data, 5 of 11 outcomes fall near the core point and another 3 outcomes lie in the inner polygon just below it. While the fit of the core is far from ideal, even here it appears to have considerable predictive power. We conjecture that the reason that the veto player does not obtain his ideal point as outcome is that motion toward the veto player requires continued shifts in the composition of the winning coalition.

3. For games where players are equally weighted there is reason to believe that the area of the minmax set will shrink as the number of voters increases (Demange 1982; cf. Feld, Grofman, and Miller 1985).

4. Of course, there are decision costs to searching out new alternatives; thus we may never get to the minmax set. The work of Kramer (1977) provides one plausible agenda process that will lead us toward the minmax set. Kramer (1977) proposed that at each iteration of a sequential decision process actors would seek out the alternative that defeats the status quo by the greatest margin. Kramer's discussion is couched in terms of two-candidate competition, but it can just as easily be restated as a process directed toward finding consensus (i.e., one seeking outcomes that beat the status quo by the largest margin).

5. An exception would be where the group is operating under a de jure unanimity rule. Here the group pressure would probably act so as to exert pressure on "deviants" from the group consensus to reduce the effective rule to prevent minority veto power. This appears to be true in juries (Grofman 1981).

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Norman Schofield is Professor of Economics, Washington University, St. Louis, MO 63130 and was Visiting Research Political Scientist, School of Social Sciences, University of California, Irvine, CA 92717 during 1984–87.

Bernard Grofman is Professor of Political Science and Social Psychology, University of California, Irvine, CA 92717.

Scott L. Feld is Professor of Sociology, State University of New York, Stony Brook, NY 11794.

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PRINCIPALS, BUREAUCRATS, AND RESPONSIVENESS IN CLEAN AIR ENFORCEMENTS

B. DAN WOOD
University of Houston

A principal-agent perspective has been employed in recent studies to rediscover the importance of democratic hierarchies in shaping public bureaucratic outputs. I test the robustness of the hierarchy model for explaining outputs from an agency that has often been cast in the image of bureaucratic independence, the Environmental Protection Agency. Examining the effect of the Reagan presidency on EPA outputs for clean air, Box-Tiao models are constructed to explain shifts in the vigor of air pollution enforcements between 1977 and 1985. The analysis shows that the influence of elected institutions is limited when an agency has substantial bureaucratic resources and a zeal for their use. Moreover, under these conditions, bureaucracy can even move outputs in directions completely opposite from what a model of hierarchy would predict. The implication is that for some agencies it is necessary to give greater consideration to the agent in explaining implementation outcomes through time.

How responsive are U.S. public bureaucracies to elected political institutions? This important question of democratic control has often drawn contrasting answers. Some appraisals have emphasized the imperviousness of the "fourth branch of government" to political control, while others have accentuated an electoral-administrative hierarchy as molding policy outputs in demonstrable ways.

Bureaucratic power does lend strong centrifugal tendencies to U.S. government. Rourke (1984) described the sources of agency power relative to elected institutions as consisting of four factors: expertise, constituency, vitality, and leadership. Expertise confers bureaucratic power through superior knowledge of a problem or policy. Constituency goes to the core of political relationships through the ability of bureaucracy to mobilize political support or curb political opposition. Vitality refers to the commitment of bureaucratic personnel to job, program, or organization. Leadership is an intangible that relates to the other three variables. A good leader will bring greater expertise to an organization, effectively mobilize constituencies, and improve personnel commitment to make the organization more vital. It is the blend of these features that Rourke viewed as enabling public bureaucracies to operate in relative independence of central political authority.

Bureaucracy has been described frequently by others as a device that frustrates political direction. Elected leaders and their appointees have been seen as overly dependent on career bureaucrats. Hence, they can set the tone and image of an agency, but they can seldom change its direction (Kaufman 1981). Bureaucrats

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have more knowledge, experience, intergovernmental ties, and time than political superiors. Thus, they can resist and wait out efforts at change (Heclo 1977). And bureaucrats have a near monopoly on information about agency resources and requirements; therefore, they can maneuver to maximize budgets and slack resources (Niskanen 1971, 1975). According to these views, the ability of elected political institutions to alter bureaucratic outputs is minimal.

In contrast with these images, scholars of a quantitative persuasion have recently shown that central political institutions do make a difference in the outputs of public bureaucracies. Implementation behavior has been observed as varying consistently across time with the ideology of presidential administrations (Moe 1982; see also Beck 1982), the ideology of congressional oversight committees (Weingast and Moran 1983), and the features of the courts (Moe 1985). Model terms to represent Congress and the president are now commonly included in quantitative studies purporting to explain variations in public policy outputs, both national and federal (cf. Chubb 1985; Scholtz and Wei 1986).

Empirical evidence demonstrating a response from public bureaucracies to the major democratic institutions has generated considerable excitement, raising expectations that a theory of control can be validated based on hierarchy and an economic view of organizations (Moe 1984). The framework has been labeled by academics the principal-agent model of political-bureaucratic relations. According to the principal-agent model, central political institutions (principals) bound and mold the preferences of public bureaucracies (agents) situated lower in a policy hierarchy. The ability of central political institutions to do this depends on at least three factors: the presence and effective use of political tools of control, the resources and receptivity of the bureaucratic agents to whom the tools are applied, and the presence of crosscutting relationships that alter principal-agent responses.

Generally, the principal-agent framework casts relations between democratic principals and bureaucratic agents as ordered. Elected institutions design public bureaucracies and provide the staples for their maintenance. Although bureaucracies have interests of their own that sometimes motivate circumvention of central direction, their shirking tendencies can be overcome. Elected institutions appoint leadership, manipulate personnel, control organizational structure, provide vital resources, employ oversight mechanisms, and exercise direct authority to evoke a response from bureaucratic agents. That is, democratic institutions are able to alter the preferences of bureaucracies systematically, making the outcomes of the implementation policy process more predictable.

The Research Focus and Vehicle

I will test the adequacy of a principal-agent framework in explaining outputs from an agency that should (if any does) conform to the model of bureaucratic autonomy: the Environmental Protection Agency. EPA provides a formidable test for the model since it traditionally has possessed all of the features which vest an agency with power. The purpose of the analysis will be to specify more precisely the conditions under which democratic hierarchy effectively shapes implementation outcomes.

Not only is the research vehicle for this evaluation different from that of previous studies of principal-agent relations, but the methods are also different. They are different in two fundamental respects. First, they are distinctive in the form of the data. Others have examined these relations with longitudinal methods using

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data divided by annual intervals (e.g., Chubb 1985; Moe 1982; Scholtz and Wei 1986; Weingast and Moran 1983). Using time series impact assessment, nonlinear probability, or pooled regression methods they have found relationships between elected political institutions and bureaucratic outputs that suggest the importance of hierarchy. This research will also use longitudinal methods, but unlike previous work the data will be divided by monthly intervals to "zoom in" more closely on the policy process. Finely divided data are expected to yield better resolution for sensing causal sequences relevant to hypotheses.

Providing greater longitudinal resolution enables the analysis to differ from previous work in another more important respect. The purpose will be to learn more than simply *whether* public bureaucracies respond to elected political institutions. It will also be to discover *when, how,* and *why* public bureaucracies respond to elected political institutions. More specifically, the following questions are addressed here: Does the timing of public policy change suggest that bureaucratic agents respond to political principals acting independently or acting in tandem? When public bureaucracies do respond, is the response initiated by symbolic events or by the actual application of political tools? Which political tools are most effective in evoking bureaucratic responses? How fast and with what magnitude do policy responses occur following policy initiatives? Are the responses to political initiatives always predictable for a given set of principal-agent relations? And finally, how much of the total variation in public bureaucratic behavior can be explained by considerations of hierarchy? By viewing in micro the sequence, magnitudes, and durations of changes in the implementation policy process across an interesting period of time, a better handle should be obtained on how well a principal-agent model can explain outcomes in an alleged democratic implementation hierarchy.

The method of analysis will be the time series quasi experiment (Campbell and Stanley 1966; Cook and Campbell 1979); that is, the response through time of a particular implementation bureaucracy to a particular set of policy initiatives will be determined. The focus will be on how EPA's implementation bureaucracy responded to the policy initiatives spawned by the election of 1980. The specific policy for analysis will be EPA's treatment of the Clean Air Acts. The 1980 election was followed by almost 2½ years of concerted efforts by a determined presidential administration to subdue EPA clean air enforcement efforts. This policy and these initiatives provide a rigorous test for both the bureaucratic autonomy and the principal-agent perspectives. A well-entrenched program was besieged by very strong efforts toward retrenchment at a time when conditions were seemingly ripe for change. The response to the policy initiatives holds the promise of providing answers to some theoretically interesting questions.

The Clean Air Implementation Bureaucracy

What were the features of the clean air implementation bureaucracy prior to the policy initiatives following the 1980 election? National clean air regulation was initiated in 1970 with the passage of the Clean Air Act Amendments. The 1970 legislation (and amendments in 1974 and 1977) made responsibility for clean air the federal responsibility of the Environmental Protection Agency and counterparts at the state level. This analysis will focus only on EPA.

EPA was created by executive reorganization under an order that consolidated 15 different environmental programs into one agency to deal with the environment

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as a "single, interrelated system." The structure of the organization broke from the previous trend of placing new regulatory functions in an independent multi-headed commission situated outside the executive branch. EPA was created as an executive agency with a single administrator appointed by the president. The major subdivisions of EPA were divided into areas such as enforcement, planning, research and development, hazardous material, and management. Within these divisions were offices that dealt with the major programs to be administered by EPA. In 1980, clean air implementation was handled specifically by the Office of Air, Noise, and Radiation with some tasks delegated to other functional areas. Most clean air regulation was performed by the 10 regional EPA offices. These offices contained technical specialists who performed the actual work of air pollution regulation, as well as administrators who oversaw federal clean air programs.

In spite of being created as an executive agency, EPA grew through the 1970s as an organization in substantial independence of executive authority. This independence was enabled by a number of factors. Environmental laws had clear, goal-oriented legal mandates that bound bureaucratic options. Environmental issues received the frequent attention of the media, keeping the matter salient to publics and politicians alike. Program constituencies, both aggregate and diffuse, prodded the agency to maintain vigorous environmental outputs. Many of the personnel of the organization were themselves zealous supporters and highly expert in the environmental technologies that developed through the 1970s. The agency was closely watched by several different congressional committees who viewed environmental programs as their special bailiwick, facilitating a more vigorous enforcement of the law. Because of these powerful supports, a tradition had even emerged whereby presidents selected EPA administrators to assuage constituency interests rather than for effective executive administration. By 1980 EPA had grown into an organization that literally represented the environment.

These supports were manifested through the provision of copious financial support for maintaining clean air enforcement outputs. At the time of the 1980 election, the EPA operating budget was at an all-time high of $1.35 billion (excluding sewer grants and superfund), with clean air being the largest environmental program (Office of Management and Budget 1981). In 1980, about a fourth of the total budget, $326 million, was spent directly on clean air with additional support coming from functionally supportive offices. A clean air staff of about 1,875 persons was maintained in central and regional offices for performing inspections, air quality testing, surveillance, litigation development, adjudication, and administration. At the time of the 1980 election, these personnel performed about 6,300 clean air enforcement actions annually, and monitored state agencies that performed about 21,600 clean air enforcement actions annually (Wood 1987).

These national investments in clean air had impacts in the nation that were not viewed favorably from all quarters. In the decade of the 1970s, industry spent over $80 billion on air pollution control technology (Council on Environmental Quality [CEQ] 1984, tbl. A-19), but some estimated the cost to the economy to be much higher (Weidenbaum 1979; Arthur Anderson & Co. 1979). By 1980, EPA and the states had gained substantial regulatory compliance from the more than 200 thousand air pollution sources in the United States. The Council on Environmental Quality (1981) estimated that only 27 hundred sources nationwide were not being adequately controlled. The Clean Air Acts and EPA had changed the environmental landscape but had generated considerable political opposition in

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The Reagan Policy Initiatives

Ronald Reagan made it clear from the beginning that his administration would not favor vigorous environmental regulation. Generally, he campaigned on a theme that decried the evils and inefficiencies of "big government," endorsing a shift in responsibility for many federal programs to the states and localities. "Regulatory relief" for business and industry was a major part of the Reagan agenda with environmental policies especially targeted because of their alleged large impact on the economy.

More specific to his intentions for EPA, Reagan's campaign rhetoric betrayed an insensitivity to the need for strong environmental regulation.[1] Between the election and the inauguration, extensive plans were laid down for reforming EPA and its programs.[2] That the actual administration objective was to reduce the enforcement effort was later a subject of considerable ideological debate, with the administration stating that it simply wanted to bring greater efficiency to the regulatory process. But this claim was hard to maintain after congressional staffers revealed documents demonstrating that in over 70 instances Reagan's EPA administrators told OMB in plain English that they were reducing the effort to stop pollution (U.S. Congress, House 1982, 26; see also U.S. Congress, Senate. Committee on the Environment and Public Works 1981, 59). Whatever the intentions, it is clear that several events occurring early in the Reagan presidency ought to have initiated change in EPA outputs.

The Reagan inauguration itself was a symbolic event that signaled to both EPA and pollution sources that change was eminent. It was followed immediately with decisive applications of administrative tools for bringing "regulatory relief" to industry. An antienvironmentalist attorney, Ann Gorsuch (later Burford), was nominated as EPA administrator. At the Division of Air, Noise, and Radiation, Kathleen Bennett, a former paper industry lobbyist, was given responsibility for clean air. Many other key positions within the organization were filled with administration loyalists who were antagonistic toward the agency mission. A policy analyst who worked for the Reagan transition team admitted later that "hit lists" of EPA personnel had been developed to identify those most likely to resist the changes at EPA.[3] Personnel rules were used to place those so identified in positions where they were least likely to impede reform (Kirschten 1983, 732–36; see also U.S. Congress, Senate. Committee on the Environment and Public Works 1981, 10). Through careful personnel manipulations, the White House created an administrative structure that should have been more receptive to relaxation of environmental regulation.

These early personnel initiatives were augmented by reorganizations that also aimed at changing environmental regulation. Decision making was centralized from the regional offices giving the Reagan appointees more control over enforcement activities. Additionally, the enforcement division was kept in a continual state of disarray, being reorganized every 11 weeks after the Gorsuch confirmation (Gottron 1982). Previously, referrals of environmental cases to the Justice Department had averaged about 200 annually. During the first nine months of 1981 only 50 cases were referred by EPA for prosecution, with only 12 of these coming after the Gorsuch confirmation (U.S. Congress, Senate. Committee on the Environment and Public Works 1981; Wenner 1984). The message was clear that the administration did not favor and would not support strong pollution control regulation.

All of these initiatives brought some striking changes in EPA's organizational character. There were drops in personnel

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morale and there was an exodus of program zealots and professionals. Even before the confirmation of administration appointees in May 1981, the total EPA staff had been reduced by 11%. Much larger reductions were planned for the future (Andrews 1984). It was estimated that the turnover rate at EPA during these times was about 2.7% per month, leaving a staff of conservers to run the organization (Gottron 1982). One observer remarked, "What was once a robust dynamic entity has shriveled to a gray shadow of its former self, wracked by internal dissension, run by people with little expertise in environmental issues, and dogged by a paranoia that virtually brought it to a standstill" (cited in Crandall 1982, 29).

If the EPA bureaucracy was an agent responding to initiatives promulgated singularly by an executive principal, then enforcement activity should have dropped following the Reagan inauguration and definitive applications of administrative tools. The president removed all vestiges of executive constituency from EPA programs, making it clear that he wanted them subdued. He used his formidable personnel powers to install an administrative hierarchy responsive to his wishes. He reorganized agency decision making to facilitate cooperation with his plans for "regulatory relief." And all of these initiatives resulted in an organization less able to maintain the traditional posture of bureaucratic autonomy. But these were not the end of EPA's woes.

A second related set of initiatives should also have caused retrenchment in EPA environmental outputs. Nine months after the Reagan inauguration, large budget reductions became effective for environmental programs. Congress, at the president's urging, consented to reduced fiscal supports for both EPA and the states. At EPA the operating budget was reduced by 24% for fiscal year 1982 (U.S. Congress, House 1982). These cuts had results that were specific to clean air. Between 1980 and 1983 constant dollar expenditures for air pollution regulation and monitoring declined by 42.3% (CEQ 1984, tbl. A-19). The number of personnel authorized for clean air activities declined by 31% (U.S. Congress, House. Task Force on Energy and the Environment 1982). Cuts were spread throughout the agency, decreasing EPA's ability to engage in all basic regulatory functions, as well as provide ancillary support services to EPA programs.

If the EPA bureaucracy was an agent responding to multiple political principals, Congress and the executive, then there should have been a response to the lower budget allocations beginning with fiscal year 1982. This response should have occurred for two reasons. The first and most obvious reason was that EPA no longer had the resources for conducting a vigorous environmental program. For clean air in particular, it took money to provide the personnel and equipment necessary for air pollution monitoring and abatement activities. A second equally important reason that there should have been a response was that the self-interested bureaucrats at EPA wanted to preserve the organization and its programs. Continued vigorous environmental regulation could only result in additional future budget reductions and further cripple the agency. Hence, it had become in the agent's interest to shirk responsibility for clean air regulation. A reduction in enforcement vigor was, therefore, a predictable response.

From the election and through the first 26 months of the Reagan administration, an outright assault was made on environmental programs. Congress, though certainly not antienvironmental, was more Republican than at any time since the 1950s. Additionally, it was a weak check on policy change because of the magnitude of the 1980 Reagan victory. This early reticence declined with time. EPA

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activists conducted a rear-guard action through leaks to the press and contacts with Congress. Between October 1981 and July 1982, EPA officials were called to testify before congressional committees more than 70 times to explain why they were not implementing the law (Gottron 1982). The EPA administrator, Ann Burford (previously Gorsuch), was called on at least 15 of these occasions. Matters came to a head with the toxic waste scandal when it was alleged that she was not only failing to execute the law but may also have been encouraging pollution sources to violate the law. The turmoil surrounding these matters resulted in the resignation of Ann Burford in March 1983 and, in the same month, the nomination of William Ruckelshaus as the new administrator. The Reagan administration's outright assault on environmental programs had come to a halt.

This third event, the Burford resignation (and nearly simultaneous nomination of William Ruckelshaus as a replacement), should have resulted in a positive response in the vigor of EPA's implementation efforts. The resignation was a symbolic event that signaled administration acquiescence to congressional and public demands for renewed environmental programs. Equally important, many of the obstacles that had confronted effective regulatory administration were removed by the changes that followed. There was a house cleaning at EPA with most of the initial Reagan appointees dismissed and replaced with competent professionals. Within a short time Ruckelshaus had improved morale at EPA and even convinced the administration to seek restoration of some of EPA's budget. These reinstallations of bureaucratic supports should have resulted in a gradual return of EPA to its former prominent position in promoting clean air.

Operationalizing Bureaucratic "Outputs" with Data

The Reagan inauguration and the fiscal year 1982 budget were powerful and discrete events that should have depressed policy outputs at EPA. The Burford resignation was a similarly powerful and discrete event that should have invigorated policy outputs at EPA. Because these stimuli were strong, it seems reasonable that some specific clean air bureaucratic outputs should have covaried with their incidence through time.

To evaluate the hypothesized covariations, the term *outputs* must be operationalized for EPA clean air implementation. Measurement of a vague concept like *outputs* can be problematic. In a general sense, bureaucratic outputs consist in all activities conducted to accomplish an organizational mission. In a restricted sense, however, *outputs* can be defined as those activities done to promote a core agency technology.[4] For purposes of this article, the term *outputs* is used in the more restricted sense to refer to interactions—between EPA regulators and air pollution sources—intended to secure regulatory compliance. It is these "in-the-trenches" interactions between the key regulatory actors that provide the truest measure of what policy is. Conveniently, these sorts of activities are quantifiable.

Outputs are measured by counting the number of enforcement interactions between EPA and emission sources for discrete monthly intervals with observations to be analyzed in serial form. Records of enforcement interactions have been kept since the early 1970s in EPA's Compliance Data System and were secured with a Freedom of Information Act request.[5] The obtained Milestone Report on enforcement actions is used as a data base for the response measurements that follow.

Two different measures of enforcement interactions are reasonable, based on dif-

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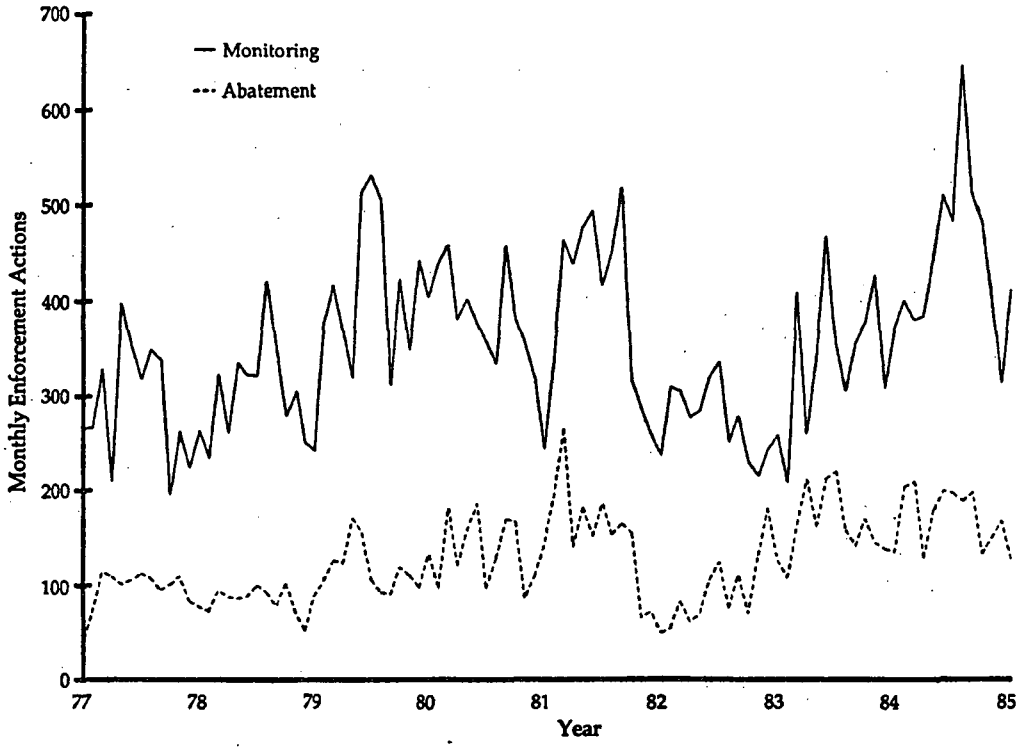
Figure 1. EPA Monitoring and Abatement Activity (1977–85)



ferent elements of the enforcement process. Monitoring activities are enforcement actions taken to assure that air pollution sources remain in compliance with the law. Abatement activities are enforcement actions taken to bring noncompliant air pollution sources into compliance with the law. The monitoring activity time series observations consist of the sum of all EPA inspections (except those for case development), compliance monitor reports, compliance tests, and routine surveillance actions. The abatement activity time series observations consist of the sum of all EPA notices of violation, administrative orders, consent orders, consent decrees, case development inspections, cases referred for litigation, and meetings for formal and informal negotiations. The two time series include data from January 1977 to January 1985. This period centers the interval containing the hypothesized policy interventions yet still retains a sizable number of preintervention and postintervention observations.[6] The time series monthly intervals correspond to regular monthly reports from EPA regions of specific actions taken to ensure compliance.

The intent of the two series is to capture the vigor with which EPA performed its enforcement functions through the period of interest. Monitoring activity is very important in causing air pollution sources to remain in compliance. The specter of being found out is usually sufficient to prevent most pollution sources from deviating too far from the law. Abatement activities, while involving fewer actions, attack the worst pollution prob-

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lems and also hold great illustrative significance for pollution sources. Each measure presents a different view of the enforcement policy process through time, which should reveal the responsiveness of EPA outputs to the hypothesized policy interventions.

Graphs of the two time series are presented in Figure 1. A cursory analysis of the pictures is interesting, but of little help when the complexity of raw time series data is considered. These data, like most other social science time series, contain extraneous components that make intuitive analysis difficult. Both time series may have seasonal components, trends, and stochastic variations unrelated to the hypothesized policy interventions. To rid the series of these undesired components and to facilitate effective analysis, methods of statistical control and modeling are necessary.

A Method for Measuring the Response

A well-developed and methodologically proven way to evaluate the effects of interventions on social processes in equilibrium is time series impact assessment (Box and Tiao 1975). These methods require the identification and estimation of mathematical components that separately describe the stochastic and deterministic variations in the series. That is,

$$Y_t = f(I_t) + N_t$$

where

Y_t = monitoring or abatement activity time series
I_t = intervention event(s) at time t
N_t = noise model for stochastic processes and trends

Modeling the series offers both an excellent way to control for the independent effects of series noise, as well as an elegant description of the series response to events of interest.

The Noise (N_t)

The best way to discover the character of extraneous noise active in a time series is empirical modeling. We care little for the causes of uninteresting variations in the data, only for the effects of certain events on series behavior. In this regard the family of models developed by Box and Jenkins (1976) called *ARIMA models* are useful.

The underlying logic behind the Box-Jenkins method is that most of the unwanted information found in data gathered across time is caused by spurious relationships originating from within the time series (autocorrelation). That is, the value of any observation at time t is almost always a function of the value of observations at time $t - n$. Because previous observations contain the effects of both external disturbances and autocorrelation, the observed time series will not present a true picture of the response at a given time t. Relevant information may be masked. Equally important, spurious effects may cause unwarranted conclusions about the hypotheses of interest. It is important, therefore, that prior to the evaluation of intervention components, time series "noise components" should be independent for each observation, normally distributed around a mean, and of constant variance (i.e., a white noise process). Box-Jenkins modeling achieves this condition through empirical identification and estimation of autocorrelation effects.

The Interventions (I_t)

The initial selection of model intervention components is the operationalization of predictions about how the series ought to respond to the interventions. Where the discovery of the series noise structure

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is a more or less atheoretical endeavor, the design of intervention components should be guided by a priori notions. That is, perceptions of the timing, magnitude, and duration of the hypothesized interventions must be considered.

There are a large number of complex forms that the response to an intervention can take. Box and Tiao (1975) suggest that many intervention responses can be modeled using transfer functions to describe the dynamic transfer from the intervention to the dependent series. An intervention, from this perspective, may be conceptualized as an exogenous input time series that acts on the dependent series in binary fashion, switching to the alternate level at the time of the intervention to disturb the dependent series equilibrium. Transfer functions are terms that describe the independent effect of the input series (I_t) on the dependent series (Y_t). It is important to consider both the form of the input series and the substance of the function operating on that series when selecting model intervention components.

The full Box-Tiao model requires, in addition to the noise model, a specification of input series and transfer functions for each discrete intervention that affects the dependent series. The initial model specification for both enforcement activity time series should, therefore, include an intervention component to represent each of the three policy interventions discussed in the preceding section. The final models should include only effective interventions since the goal of Box-Tiao modeling is to produce a parsimonious yet theoretically complete explanation for the variance in the time series process.

The Responses to the Reagan Policy Interventions

The methods described above were used to construct models of the time series for both EPA monitoring and abatement activities.

Monitoring Activity

The modeled responses of EPA monitoring outputs to the Reagan policy interventions are represented in the following equation:

$$Y_t = \omega_{o1} I_{t1} + \omega_{o2} I_{t2} + \omega_{o3} I_{t3} + \frac{a_t}{(1 - \phi_1 B)} + \mu$$

where

Y_t = EPA monitoring activity from 1977 to 1985
ω_o = intervention component transfer weights
I_{t1} = Reagan inauguration
I_{t2} = fiscal year 1982 budget reduction
I_{t3} = Burford resigns
ϕ_1 = autoregressive noise parameter
a_t = white noise disturbances
B = backshift operator such that $BY_t = Y_{t-1}$[7]
μ = mean

Parameter estimates and diagnostic statistics for the equation are contained in Table 1. The equation and parameter estimates are used to generate model-predicted values for monthly EPA monitoring activity which, along with actual values, are contained in Figure 2. The graphed predicted values present an accurate view of the movements that occurred in monitoring activities after each of the policy interventions.

How did EPA monitoring activity respond to the Reagan policy interventions? All three of the hypothesized policy interventions evoked a response in EPA monitoring outputs. But not all of the responses were in the hypothesized direction. Following the Reagan inauguration there was, surprisingly, a zero-order

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Table 1. The Impact of the Reagan Presidency on EPA Clean Air Act Monitoring Activity

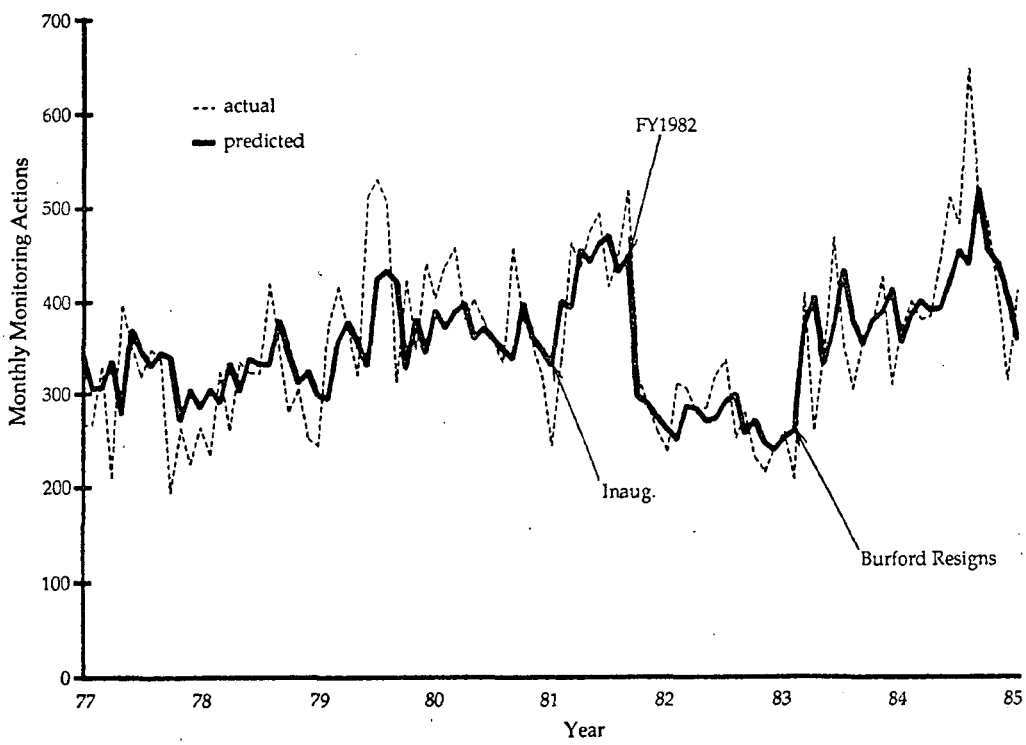
Model Component	Parameter	Monitoring Activity[a] Estimate	t Statistic
Reagan inauguration (I_{t1})[b]	ω_{o1}	105.47	2.62
FY 1982 budget (I_{t2-1})	ω_{o2}	-185.45	-4.17
Burford resigns (I_{t3})	ω_{o3}	137.87	3.95
First-order autoregressive	ϕ_1	.48	5.35
Mean	μ	343.87	19.60

Note: Measures of fit: residual mean square (noise only), 5068.10; residual mean square (full model), 4223.39 (% change = 16.67); autocorrelation of residuals, $Q = 18.06$ with 22 degrees of freedom (no significant lag in first 24).

[a]For the monitoring activities model, all intervention series (I_t) were step inputs.

[b]No transfer function δ_1 parameters were significant, indicating very abrupt responses.

Figure 2. Model-Predicted EPA Monitoring Activity (1977–85)



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increase in EPA monitoring activity of about 105 monitoring actions per month. This increase constituted a change of about 30% over the average number of monthly monitoring actions that had characterized the previous Democratic administration. This means that following the Reagan inauguration, the EPA bureaucracy bucked the administration and used its slack resources to substantially increase surveillance of pollution sources.

After the implementation of the fiscal year 1982 budget, EPA no longer maintained the high level of monitoring activity. The number of monthly monitoring actions dropped by about 185, a decline of 41% from the number being conducted from February through September of 1981. This reflected a decline of 23% from the average number in the previous Democratic administration. The fiscal year 1982 budget had both reduced EPA resources for air pollution monitoring and displaced bureaucratic interests toward avoiding further reductions. Congress, through the budget, had signaled tacit consent to the new directions at EPA. Accordingly, EPA responded to signals from dual political principals by maintaining monitoring activity at very low levels until the Burford resignation in March 1983.

Following the Burford resignation, there was an immediate zero-order jump in the number of monthly monitoring actions of about 138, an increase of about 50% from the depressed level that had existed. The change reflected an increase of 16% over the pre-Reagan average number of monthly monitoring actions; that is, following the resignation, monitoring levels were actually somewhat higher than they had been during the supposedly friendlier Democratic administration. The committed bureaucrats who remained at EPA had again found slack resources to restore and intensify their surveillance activities, signaling a renewed EPA interest in detecting noncompliant pollution sources.

The causal mechanisms that enabled the jump in monitoring activity following the Burford resignation are not patently clear from an analysis of the data. Public support, both diffuse and aggregated, for environmental regulation had been strong throughout the period of the policy interventions. It had become obvious many months earlier in congressional hearings that legislative consent to an emasculated environmental policy was not present, but there was no increase in funding for EPA activities until October 1983. Nevertheless, a response occurred only after the Burford resignation. The logical interpretation is that EPA bureaucrats, long subdued by a hostile taskmaster but short on resources, engaged in a celebration. The zealous but risk-averse bureaucrats at EPA viewed the resignation as a cue that it was permissible for them to resume vigorous regulation of the environment without fear of retribution. They moved decisively to reestablish regulatory control of air pollution sources.

Increased monitoring activity was one mechanism EPA used to reestablish its core technology following the Burford resignation. But there were limits to what could be accomplished in enforcements with more surveillance. Moreover, because EPA resources were limited for so long, there were deficiencies in the quality of monitoring activities that could be conducted immediately. For one thing, they initially consisted of nonresource intensive activities such as readings of compliance monitors and more numerous tests. There was no significant increase in the personnel or equipment available for compliance inspections until October 1983 when modest budget increases were implemented.[8]

Increased monitoring activity was not the only device used by EPA regulators for reestablishing the clean air program following the Burford resignation. More-

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Table 2. The Impact of the Reagan Presidency on EPA Clean Air Act Abatement Activity

Model Component	Parameter	Abatement Activity[a] Estimate	t Statistic
Reagan inauguration (I_{t1})	ω_{o1}	84.83	3.47
	δ_{11}	.93	14.00
FY 1982 budget (I_{t2-1})[b]	ω_{o2}	104.80	-3.70
	δ_{12}	.87	10.64
Burford resigns (I_{t3})	ω_{o3}	63.83	3.50
	δ_{13}	.99	60.26
First-order autoregressive	ϕ_1	.25	2.41
Mean	μ	111.42	18.94

Note: Measures of fit: residual mean square (noise only), 1257.09; residual mean square (full model), 914.23 (% change = 27.27); autocorrelation of residuals, Q = 17.05 with 22 degrees of freedom (no significant lag in first 24).

[a]For the abatement activities model, all intervention series inputs (I_t) were pulsed.

[b]For the abatement activities model, the FY 1982 intervention series was lagged one month.

over, the model described below for EPA abatement activities suggests that changes in monitoring levels were only a secondary device.

Abatement Activity

The modeled responses of EPA abatement outputs to the Reagan policy interventions are represented in the following equation:

$$Y_t = \frac{\omega_{o1}}{(1 - \delta_{11}B)} I_{t1} + \frac{\omega_{o2}}{(1 - \delta_{12}B)} I_{t2-1} + \frac{\omega_{o3}}{(1 - \delta_{13}B)} I_{t3} + \frac{a_t}{(1 - \phi_1 B)} + \mu$$

where

Y_t = EPA abatement activity from 1977 to 1985
ω_o = intervention component transfer weights
δ_1 = transfer function rate of change weights
I_{t1} = Reagan inauguration
I_{t2} = fiscal year 1982 budget reduction
I_{t3} = Burford resigns
ϕ_1 = autoregressive noise parameter
a_t = white noise disturbances
B = backshift operator such that $BY_t = Y_{t-1}$
μ = mean

Parameter estimates and diagnostic statistics for the equation are contained in Table 2. The equation and parameter estimates are used to generate predicted values for monthly EPA abatement activity that, along with actual values, are contained in Figure 3. The graphed predicted values present an accurate view of the movements in EPA abatement activities after each of the policy interventions.

The interventions and polarities of responses that were effective for the preceding monitoring activities model were also characteristic of the abatement activities model. But the abatement activities model was distinctive in the much larger magnitude of the responses and the presence of significant first-order model intervention parameters (δ_1).

What do these modeling results say about EPA clean air abatement activities after the Reagan policy interventions?

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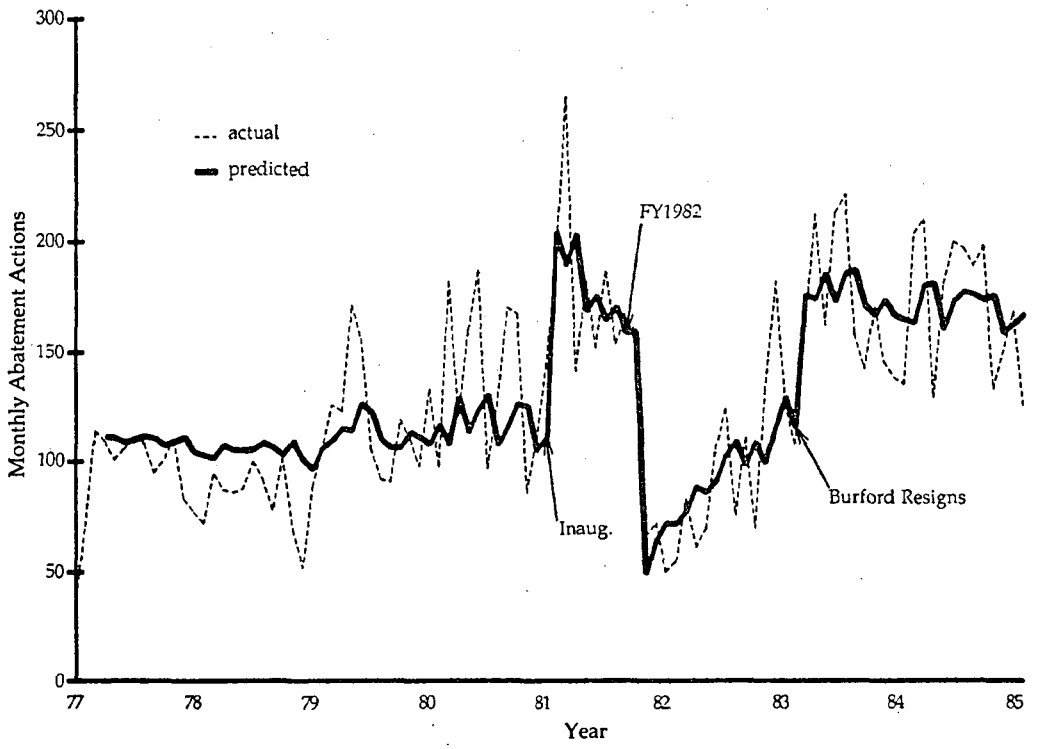
Figure 3. Model-Predicted EPA Abatement Activity (1977–85)



Following the Reagan inauguration there was an abrupt and very large increase in the number of monthly abatement actions. The model shows that EPA conducted about 85 more abatement actions than usual in the first month after the inauguration, a rise of about 76% over the number that had characterized the previous Democratic administration. The full magnitude of the increase was temporary, immediately entering a gradual decline toward the preintervention level. The rate at which EPA abatement activity moved downward from the peak is reflected in the δ_{11} parameter for the Reagan inauguration model term (I_{t1}). With first-order transfer functions, the δ_1 parameter is constrained between the interval ± 1, with values near unity indicating a very slow rate of change. Accordingly, the model shows that EPA abatement activities would have remained vigorous, falling only very slowly, if there had been no change stimulus at fiscal year 1982.

Following the fiscal year 1982 budget, EPA abatement activity declined by about 105 abatement actions per month, a drop of 69% from the existing numbers. The drop sent EPA abatement outputs to a low that was 56% below the average monthly levels of the previous Democratic administration. The decline in activity was delayed one month from the hypothesized time, occurring in November 1981. The delay hints at the transitory character of the bureaucratic response for abatements. The impermanence of the change is confirmed by observing the gradual resurgence of abatement activities following the large initial negative

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response. The rate at which abatement activities returned following the fiscal year 1982 budget cut is described by the δ_{12} parameter for the intervention (I_{t2-1}). Significantly, by the time of the Burford resignation in March 1983, the number of monthly abatements had been restored to a level about equal to that which prevailed during the previous Democratic administration.

After the Burford resignation, EPA abatement activities increased abruptly by about 64 actions per month to rise 52% above the existing level. This change placed the average number of monthly abatement actions at 58% above the numbers that prevailed under the earlier Democratic administration. The response to the Burford resignation was modeled as a temporary change. But the rate of decay back toward the preintervention level was exceedingly slow, as reflected in the large value of δ_{13} for the intervention term (I_{t3}). The implication is that for a long time following the Burford resignation, an EPA bureaucracy with limited fiscal support used abatements, a non-resource-intensive activity (i.e., little extra effort is required to begin issuing more notices of violation or administrative orders) as a wedge for bolstering EPA clean air primacy. And they used them intensively since the numbers were maintained for the remainder of the series at levels higher than for any similar period in the history of the agency.

Considering the EPA abatement activity model as a whole, the model suggests that clean air abatement actions were manipulated by EPA bureaucrats at crucial times to boost regulatory control. Specifically, after the Reagan inauguration the number of EPA abatement actions increased when EPA bureaucrats wanted to reverse the notion that noncompliant behavior was acceptable. They increased again when EPA bureaucrats wanted to do the same following the Burford resignation. Moreover, since abatement outputs increased steadily after the fiscal year 1982 budget reductions, it is reasonable to conclude that bureaucratic inertia had an effect even during a period of limited resources and severe political constraints.

Implications for a Theory of Hierarchy

The statistical analyses described above are more than simply an exercise in policy modeling. My expressed purpose is to test the adequacy of a principal-agent framework in explaining outcomes through time in an implementation policy process. The vehicle through which relations are being analyzed, EPA clean air policy, provides a rigorous test for the theory. The preintervention bureaucracy was archetypical of a case where the agent ought to have been able to operate in relative independence of executive authority. But the intervention stimuli were very strong, guaranteeing large responses. The findings reported above demonstrate clearly that considerations of hierarchy, although important, have obvious limitations for explaining outcomes in some implementation policy processes.

For clean air, a principal-agent model would predict that the Reagan administration, given the most Republican Congress since the 1950s and extraordinary political influence, should have been able to shift the preferences of the environmental bureaucracy. All of the available tools of administrative control were applied toward moving EPA away from vigorous implementation of the law. But the data analysis shows that in the end EPA's revealed preferences were completely opposite from what the model predicted. After the Burford resignation and continuing for the duration of the data, enforcements were pursued more vigorously than at any time in the agency history and in a manner inconsistent with the

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ideological dispositions of elected political institutions. Moreover, throughout the observed policy process there were serious failures of explanation for considerations of hierarchy.

A model of hierarchy does not explain the increase in EPA enforcements that occurred in the months following the Reagan inauguration. In spite of the considerable effort the Reagan administration applied toward reining in enforcements at EPA, alleged bureaucratic agents seem to have acted alone, contrary to administration wishes, to increase enforcements well above the numbers that had existed during the previous Democratic administration. Nor does hierarchy explain why, after the fiscal year 1982 budget, the number of abatement activities began climbing from their depressed level to return to pre-Reagan-era levels by the time of the Burford resignation. The proclivity of EPA regulators to regulate seems to have been a factor negating the administration's ability to keep clean air enforcements to a minimum. A model of hierarchy does not explain the magnitude and decisiveness with which enforcement vigor returned following the Burford resignation in March 1983. After this intervention, abatement activities moved to a much higher level than characterized the earlier Democratic administration. Furthermore, a model of hierarchy does not explain why EPA abatement outputs remained at the higher level to 1985 with bureaucratic supports which had not grown significantly over those of the past.

These limits on the ability of hierarchy to explain implementation outcomes do not mean that principal-agent relations are irrelevant. They did make a difference in EPA's clean air outputs, since for both measures of enforcement activity there were drops following the fiscal year 1982 budget reductions. But they were only temporary, and there is no evidence in the causal sequence that the changes were an effect of executive authority operating directly on the EPA organization. Reagan's appointments, personnel changes, reorganizations, removal of executive constituency, and other initiatives intended to bring the organization into line were seemingly ineffective in initiating "regulatory relief" for industry. Executive authority was only made effective through the power of the purse. That is, Congress mediated the relationship between the executive and the bureaucracy by giving its tacit approval to administration policies with large budget reductions.[9] When that tacit approval was removed by the forced resignation of the EPA administrator in March 1983, executive authority returned to a condition of impotence. EPA followed a new course, secure in the knowledge that there would be no future budget reductions comparable to those in fiscal year 1982. Outputs jumped to and remained at levels higher than would have been predicted given the ideological predilections of the Reagan administration. The image produced by these findings is that of a hierarchy effective only when there is a consensus of multiple principals (a "coarchy"), with bureaucracy very important in substantive outcomees.

One possible explanation for the failure of the model to explain clean air outputs adequately is that there were multiple principals operating on EPA behavior. This hypothesis would suggest that clean air outputs were responsive to a supportive Congress rather than to the executive. But the congressional responsiveness hypothesis likewise does a poor job of explaining important variations in clean air outputs. At the time of the Reagan inauguration, when enforcements increased sharply, there was no stimulus sent by the Congress or its committees to cause the increase. The more conservative Congress was, in fact, reticent when faced with a president who had won by an electoral landslide. Similarly, when abatement outputs began increasing from their

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depressed levels after fiscal year 1982, it hardly seems likely that a Congress that had just approved large budget reductions was responsible for the movement. Furthermore, when enforcements increased dramatically following the Burford resignation, the timing and magnitude of the changes do not suggest that a shift in congressional influence was wholly responsible. The 1982 congressional election did dilute the perception of a mandate that had prevailed since the 1980 Reagan victory. But there was no significant alteration of the oversight committees, and there was no increase in EPA appropriations until fiscal year 1984. The decisiveness of the changes in March 1983 suggests that EPA bureaucrats, while perhaps enabled by intense external interest, took advantage of the situation to increase enforcement outputs to unprecedented levels. Moreover, the proposition that a Republican Congress would spur enforcement activity to levels more vigorous than in any period in agency history is fully inconsistent with previous research on these relations. The congressional responsiveness hypothesis is, then, an unsatisfactory explanation for the variations through time in EPA clean air outputs.

Why does a principal-agent model fail to explain the longitudinal variations in EPA clean air outputs? Simply put, the model as previously applied does not give sufficient weight to bureaucratic interests in shaping policy outputs.[10] The ability of elected principals to shape outputs depends not only on the effective application of political tools of control, but also on the resources and receptivity of bureaucracy to behavioral modification. A failure of control can result at either the principal or the agent end of relations. Elected institutions may manipulate political controls poorly and fail to evoke a response (Waterman 1987). Likewise, a bureaucracy imbued with substantial resources and a willingness to use them may also frustrate political direction. What has been illustrated here is a limit of political control, a case where manipulation not only was unsuccessful but backfired to move outputs in opposite directions.

Significantly, EPA bureaucrats acted at all times to protect their interests in promulgating the agency core technology. Sometimes this entailed restraining clean air enforcements to avoid further budget reductions. At other times it entailed moving outputs upward to preserve control over regulated air pollution sources. In the latter cases, the changes at EPA came to warrant largely and decisively the conclusion that bureaucracy was an important independent influence. The basal implication is that bureaucracies are more than receptors of policy stimuli emanating from an electoral administrative hierarchy. Rather, they are themselves responsible for much of the variation and substance of public policy through time.

A Methodological Concern for Future Research

An important methodological consideration flows from the findings presented here. This research is different not only because of the objects taken for analysis but also because of the way in which the data are analyzed. Previous quantitative examinations of the relation between central political institutions and bureaucratic outputs (Chubb 1985; Moe 1982; Scholtz and Wei 1986; Weingast and Moran 1983) employed longitudinal designs with data divided by annual intervals. Annual data reveal only the tendencies of a policy process and mask the details that should flow from serial analysis.[11] This research employs a longitudinal design with finely divided intervals to capture an empirically rich dynamic underlying the process tendencies. Dividing a time series by small intervals allows

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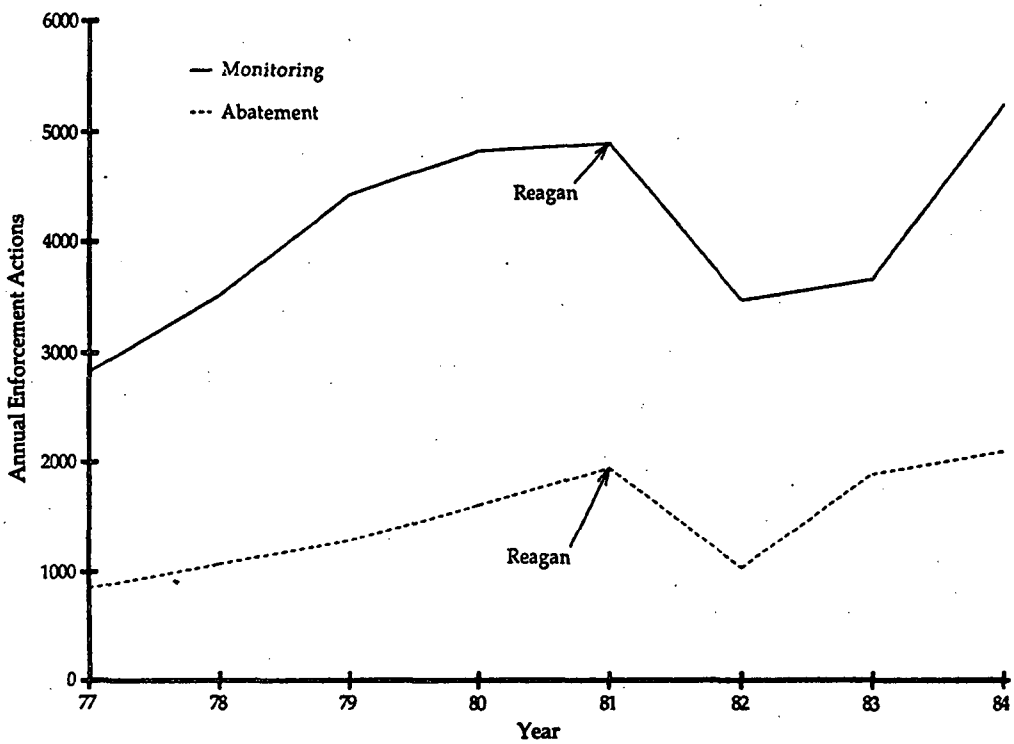
Figure 4. EPA Monitoring and Abatement Activity (1977–85)



observation in micro to more readily detect the sequence, magnitudes, and durations of policy changes. The insight that a knowledge of these factors gives is invaluable for providing answers about when, how, and why changes occur in political behavior. As an illustration of this point it is informative to compare the implications that flow from the clean air enforcement data aggregated annually (see Figure 4) with those coming from the same data divided by monthly intervals (see Figures 2 and 3). A critical difference between this research and previous research lies, then, in what is being explained: tendencies or multiple outcomes.

A caution should be issued of the danger in making inferences from longitudinal data that have poor resolution. Not only is the underlying dynamic obscured, causing the analyst potentially to miss important movements, but the prospect is enlarged for making incorrect judgments due to problems of history (Campbell and Stanley, 1966, 39; Freeman 1983, 337). In the clean air case, for instance, if the data were aggregated annually, rather than monthly, the results of this analysis would not have differed markedly from previous research extolling the importance of electoral-bureaucratic hierarchies. The tendency of both time series was for outputs to be depressed in the early years of the Reagan presidency. Most of the time series variance was contained in the drop in enforcements after 1981.[12] Because of the magnitude of the changes and the low data resolution, the other variations in the series would have been lost. The conclusion would have been that elected principals were effective in altering the prefer-

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ences of bureaucratic agents at EPA. This conclusion, based on the process tendency, would have been parsimonious but not descriptive of what really occurred.

In observing a policy process longitudinally, it is important to consider carefully what can be resolved by a particular set of time series data, tendencies or multiple outcomes. Research goals should be set accordingly within limits imposed by the data. Where the data are selectable, choice of time series intervals should be made in awareness that decisions are akin to those that typically must be made between parsimony and substantive accuracy. Data with low resolution allow explanations of process tendencies, but may miss movements important to theory. Data with high resolution allow better explanation for the many variations occurring in policy through time but may require many variables to account for a complex process. Where it is not possible to select the data for analysis, research conclusions should be tempered by an awareness of the limits imposed by these considerations.

Conclusion

This research bridges a gap between two contrasting bodies of theoretical literature on the relation between elected political institutions and public bureaucracy. The tradition that casts public bureaucracy as a pervasive influence, making policy inertial, is found partially correct since bureaucracy made a significant difference in EPA clean air outputs. On the other hand, the emerging tradition that casts the outputs of bureaucracies as predictable through consideration of democratic hierarchies is also found partially correct since elected principals were able (if only temporarily) to shape policy outputs. The centrifugal influence of bureaucracy and the centralizing influence of hierarchies are both important, requiring consideration in a theory of political control. The extremes of the two views may be reconcilable under a principal-agent framework if more emphasis is given to the agent side of relations in future research. But it must be recognized that public bureaucracies are not simply instruments of the constitutional organs of government manipulated through behavioral incentives.

Theories of bureaucracy must make clear the dual capacity in which administrative institutions operate. Bureaucracies translate current events and ideologies into change, making government responsive to popular initiatives. They also manifest past events and ideologies, thereby insulating government from the continual progression of passing stimuli. They are reflections of both policy in the present and in the past. In this dual role, they operate as *agents of the law* who, by virtue of delegated authority, are transformed into quasi principals. Consequently, they can themselves be an important source of the variation and substance of implementation outputs through time.

This does not mean that democratic controls have failed. Public bureaucracies are legitimate principals charged with the obligations of providing responsiveness and stability. Like elected institutions of government, they have representational tasks. They represent all of those static coalitions from the past that successfully had their policy ambitions transformed into law. Bureaucracy derives its legitimate claim to principality from the laws and regulations that flowed from past coalitions. It often has inherent interests in faithfully preserving the law. Radical changes in direction seldom come unless new laws are passed. At all times, responsiveness depends on the outcomes of bidirectional interactions that may or may

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not demonstrate a hierarchical aspect. Bureaucratic principals may even compete, at times, with elected principals for control of policy to move outputs in unanticipated directions. Put simply, bureaucracies are subject to limited manipulation by elected institutions, but their responsiveness is bounded by a legitimate representational task. And they do some manipulating of their own.

Notes

This research was supported by National Science Foundation Grant SES 8700615. I thank James A. Stimson for his assistance on this project and his tutelage while I was a graduate student at the University of Houston. I also want to thank Richard Barke, Malcolm Goggin, and Richard Waterman for helpful comments on earlier versions.

1. Consider, for example, the following materials paraphrased from Kraft (1984, 35–43): Early in the campaign, Reagan stated that air pollution in the United States had been "substantially controlled." In another speech he claimed that trees and plants were responsible for more pollution than all of U.S. industry and went on to call for reform of the Clean Air Acts to aid the steel industry. On another occasion he said that if EPA had its way, "you and I would have to live in rabbit holes." He referred to the oil slicks off the California coast as having been considered healthful at one time in U.S. history. But the most publicized of these gaffes was the Mount St. Helens statement in which Reagan claimed that the volcano emitted more sulfur dioxide than had come from all automobiles in the last 10 years.

2. During the transition period, plans were formulated by the conservative Heritage Foundation that described EPA as a hotbed of economic inefficiency and advocated "the most comprehensive and far-reaching programs of regulatory reform ever undertaken by any administration in the nation's history" (Cordia and Hinish 1981; for a good summary see Kraft 1984). Not all observers agree that these recommendations were the ones that drove the early administration of EPA (but see William E. Schmidt, "Denver Lawyer's Role in EPA Decisions Is Focus of Inquiry by Congress," *New York Times*, 26 February 1983; see also B. Drummond Ayres, Jr., "Conservatives Bid Reagan Cut More," *New York Times*, 22 January 1983; and Russell W. Peterson, "Laissez Faire Landscape," *New York Times Magazine*, 31 October 1982). Nevertheless, there is striking similarity between what was contained in the foundation's suggestions and what actually occurred at EPA.

3. Stuart Taylor, Jr., "Ex-EPA Aide Says He Drew Up 'Pro and Con' Lists on Personnel," *New York Times*, 17 March 1983.

4. The term *core technology* is borrowed from Thompson (1967), who develops a theory of organizations based on the concept.

5. The Freedom of Information Act Petition was submitted to the EPA Office of Air, Noise, and Radiation, Stationary Source Division. The data were retrieved in the form of a printed computer listing of enforcements from all 54 states and territories within the 10 EPA regions.

6. There were 102 observations, with the inauguration falling on the 49th and the Burford resignation falling on the 75th.

7. The use of backshift notation is conventional shorthand in statistical time series analysis. For a short and very simple discussion of backshift algebra see McCleary and Hay 1980, 45–48.

8. In a separate analysis of compliance inspections only, there was found a significant first-order step intervention in October 1983 when the first Ruckelshaus budget was implemented. About $165.5 million was restored to EPA's operating budget at this time, of which an unknown amount found its way into promoting clean air.

9. Note that since political influence was manifested through the budget, a shared political tool, there are some real difficulties in defining just which principal was responsible for altering bureaucratic behavior. Who made the difference: the executive, Congress, or both? The problem involves issues of theory that cannot be resolved by statistical analysis (or any other form of analysis). It also involves difficulties for statistical analysis in parameter interpretation since neither executive nor congressional influence is independent. One solution to this problem could be to use a single-model term from the multiplicatiaon of the two variables. This specification captures the substance of the relation described below as *coarchy*.

10. Bendor and Moe (1985, 1986) do explicitly include bureaucracy in their dynamic model of these interactions. At this point, however, no one has effectively operationalized agent interests in empirical modeling of these relations.

11. The discussion in this section is applicable whether the data are analyzed with regular time series methods, pooled methods, or nonlinear probability models with variables divided by annual intervals.

12. It is worth a special note to caution about those longitudinal methods (e.g., pooled regression) that do not force the analyst to look at the data. A model term may be theoretically satisfying, explain

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substantial amounts of variance, be statistically significant, and still miss what actually occurred.

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B. Dan Wood is Visiting Assistant Professor of Political Science, University of Houston, University Park, Houston, TX 77004.

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CONTROVERSY

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DO U.S. SENATORS MODERATE STRATEGICALLY?

Do U.S. senators adjust their policy positions or voting behavior—engage in "strategic moderation"—in their quest for reelection? In the June 1986 issue of this Review, *Gerald Wright and Michael Berkman sought to demonstrate that Senate incumbents moderate their ideological positions as elections near. This endeavor was part of their larger effort to show the importance of policy issues in the selection of members of Congress. Robert Bernstein takes the view that the claims about strategic moderation rest on methodological flaws. But Wright and Berkman argue that most investigators agree on the general direction of senatorial candidate behavior. The controversy turns on conception and interpretation of analytical results.*

To support their argument "that policy issues play an important role in the selection of members of Congress," Gerald Wright and Michael Berkman attempt to show that Senators engage in "strategic moderation," that is, that Senators shift to more moderate positions in the last two to three years of their terms in order to increase their prospects for reelection. In so doing, Wright and Berkman enter an ongoing debate on whether there is empirical evidence of strategic moderation (Elling 1982; Thomas 1985). I think measurement flaws in their research leave that debate unresolved.

Do Senators Engage in Strategic Moderation?

To demonstrate that senators "moderate later in their terms to increase their election appeal" (p. 575), Wright and Berkman compare the mean policy conservatism of senators up for reelection in 1982 with the *mean* policy conservatism of similarly situated senators not up for reelection. That way of measuring "strategic moderation" has two serious flaws: (1) the *mean* conservatism score for a group is *not* a measure of how moderate group members are; and (2) a static 1982 comparison cannot distinguish between senators who became more moderate as the election approached and those who were moderate throughout their careers.

Regarding the first flaw, a mean rating of *neutral* on a conservative-liberal index for a group of senators can reflect moderation by every senator in the group, but it can also reflect counterbalancing extremists. If one candidate has a score of +5 (most conservative on the index used by Wright and Berkman), and another a score of −5 (most liberal), the candidates' party gets a score of 0 (perfect moderation); but that party score does not reflect any "strategic moderation" by party candidates.

If one is using a conservatism scale, a better measure of the moderation of each senator is the absolute value of the difference between each senator's score and the scale score for neutrality (Elling 1982). The mean of those absolute values can then be used as a summary measure for how moderate a group of senators is.[1]

Regarding the second flaw, a group of senators running for reelection can be

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more moderate than a similarly situated group not running for reelection without that relative moderation being "strategic"; that difference in moderation might always have been characteristic of the groups. A better measure of strategic moderation is to compare moderation scores from different years to see the extent to which those running and those not running shift positions as the election approaches. Since Wright and Berkman argue that strategic moderation occurs in the last two to three years before an election, a comparison of third-year and sixth-year moderation scores will disclose any strategic moderation that may occur.[2]

The over-time comparison should really highlight strategic moderation. Not only will those running for reelection be *more* moderate than they were three years earlier because of strategic moderation, but those not running will tend to be *less* moderate than three years earlier, when about half of them had faced reelection.

Measuring Strategic Moderation

For reasons detailed by Elling (1982, 79), moderation in each year is probably best measured by deviation from the midpoint of one of the standard measures of senators' ideology (Americans for Democratic Action or Americans for Constitutional Action [ACA] ratings). The ACA ratings, because they do not penalize for absences, are preferable and are used in the analysis below. To control for changes in the construction of the ACA scales, it is necessary to observe *relative shifts* in moderation scores by both those running for reelection and those not running for reelection.

While there may be some debate over which is the best control group for comparison with those running for reelection, the best case can be made for those senators not running for reelection *but from the same states* as those running for reelection. That comparison eliminates problems stemming from comparison with a group of senators whose constituencies differ geographically, ethnically, occupationally, and so on from the constituencies of those running. Limiting the control group to senators serving during the entire period also eliminates the possibility that changes in moderation scores might be caused by turnover. Table 1 compares changes in moderation scores for those running for reelection with same-state senators not running for reelection for those states in which the two senators were the same throughout the time period.[3]

Table 1 shows very limited evidence of strategic moderation. As their terms neared their end, Democrats standing for reelection in 1982 did moderate their positions substantially relative to their control group. However, Republicans standing for reelection in both 1982 and 1984 showed only slightly more moderation (or less extremism) than did their control groups, and Democrats running in 1984 showed less moderation (or more extremism) than did those not running.

Given that the relative changes in moderation scores should overstate any evidence of strategic moderation and given the sometimes weak or contradictory findings, there is little support for the hypothesis that senators moderate their positions as reelection time approaches.

Why There Is So Little Evidence of Strategic Moderation

It should not be surprising that there is so little evidence of strategic moderation. The argument for strategic moderation is based on an oversimplification of theories derived from spatial analysis. Even if one assumes that senators *shift* ideologies in order to enhance reelection prospects, it does not follow that senators will *moderate* their positions unless the senators assume that the median voters in their

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Table 1. Mean Moderation Scores for Senators Running for Reelection and Same-State Senators Not Running

	Democrats		Republicans	
Time Period	Running	Not Running	Running	Not Running
For 1982				
Moderation (1982)	14.2	27.1	21.5	27.2
Moderation (1979)	27.3	32.7	27.1	30.5
Change (1979–82)	−13.1	−5.6	−5.6	−3.3
Number of cases	13	11	8	10
For 1984				
Moderation (1984)	21.9	25.9	32.3	36.3
Moderation (1981)	15.6	20.7	20.6	21.7
Change (1981–84)	+6.3	+5.2	+11.7	+14.6
Number of cases	11	11	15	15

districts are (1) more moderate than they are and (2) likely to be closer to their future opponents' ideologies than to their own. Such would never be the case for conservative Democrats expecting to face even more conservative Republicans or for liberal Republicans expecting to face even more liberal Democrats. If they are already positioned between the median voter and the potential opposition, movement towards the center would be expected to cost votes. If the median voter is positioned between them and the potential opposition, or beyond the potential opposition, only a shift towards a more extreme position would be expected to gain votes. Thomas (1985) empirically verified that argument by finding that conservative Democrats and liberal Republicans do not moderate as elections approach.[4]

Strategic moderation would not even be expected to enhance the election prospects of all liberal Democrats and conservative Republicans. However, restricting comparisons to just liberal Democrats and conservative Republicans does increase the probability of finding evidence of some strategic moderation. Table 2 shows the results of those restricted comparisons for senators running in 1982 and 1984 and their appropriate control groups.

As theory would suggest, the evidence of strategic moderation is somewhat stronger when the sample is restricted to those most likely to expect to profit from such moderation. Those running shifted to more moderate positions relative to their respective control groups for both parties, both years. The *N*s are small, as is the relative shift by Democrats running in 1984. However, the results do suggest some strategic shifting to more moderate positions by liberal Democrats and conservative Republicans.

Are Moderates More Likely To Be Reelected?

The hypothesis that senators will moderate their positions is in part based on the hypothesis that moderates are more likely to get reelected. While Wright and Berkman do not test that hypothesis directly, they present an empirically derived equation that indicates just the opposite: that senators running for reelection would gain the most votes by taking extreme positions.

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Table 2. Mean Moderation Scores for Liberal Democratic and Conservative Republican Senators Running for Reelection and Same-State Senators Not Running

Time Period	Democrats		Republicans	
	Running	Not Running	Running	Not Running
For 1982				
Moderation (1982)	13.0	26.5	23.0	27.0
Moderation (1979)	28.3	35.0	33.0	30.3
Change (1979–82)	−15.3	−8.5	−10.0	−3.3
Number of cases	13	11	8	10
For 1984				
Moderation (1984)	20.6	29.0	36.9	42.4
Moderation (1981)	19.4	26.8	26.4	24.6
Change (1981–84)	+.8	+2.2	+10.5	+17.8
Number of cases	11	11	15	15

Wright and Berkman present an equation (number 2) showing that the likelihood of a person casting a Republican vote *(V)* is a function of *the product* of the person's ideology *(I)* and the candidate's ideology *(C)*.[5] Once other variables are controlled, $V = .054\ IC$, for the Republican candidate's ideology, and $V = -.038\ IC$, for the Democrat's ideology.[6]

In their equation, I takes the value of −1 for liberals, 0 for moderates, and 1 for conservatives. Thus $.054\ IC$ (or $-.038\ IC$) is always 0 for moderates. For moderates, the equation indicates that the probability of a Republican vote (or a Democratic vote) is completely unaffected by the extremism of the candidate.

For liberals and conservatives, however, the candidate's ideology does affect the probability of a Republican vote. Candidate ideology is coded from +5 (most conservative) to −5 (most liberal). If a Republican candidate adopts an extreme conservative position, the candidate increases the probability of a Republican vote by (.054)(1)(5) for each conservative voter and decreases the probability of a Republican vote by the same amount for each liberal voter.[7]

If there are more conservatives than liberals in the district, the Republican candidate maximizes the number of Republican votes (maximizes the sum of $.054\ IC$ over all voters) by taking the most extreme conservative position. If liberals outnumber conservatives, the Republican candidate maximizes his or her votes by taking the most extreme liberal position. Similarly, the Democratic candidate maximizes the number of Democratic votes by taking either the most extreme conservative or liberal position (thus maximizing $-V$).

If the Wright and Berkman equation is correct, candidates receive no electoral advantage from moderating their positions. Instead, they gain by taking extreme positions. This unexpected finding may be an artifact of the measures and analysis employed; there may be no electoral advantage in "strategic extremism." However, it may correctly reflect a substantially greater salience of ideology to conservative and liberal voters than to moderate voters. In either case, Wright and Berkman's individual-level analysis shows no evidence of electoral advantage to "strategic moderation."

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Strategic Behavior by Senators

Neither logic nor evidence supports the theory of widespread strategic moderation. The logic of spatial analysis suggests that moderation would be "strategic" only for some liberal Democrats and conservative Republicans. Analysis of senatorial behavior does find limited evidence of strategic moderation among just those groups of senators. However, evidence uncovered by Wright and Berkman at the individual level casts doubt upon whether moderation is in the interest of even those senators. Strategic "shifting" may prove rewarding, but that shifting need not be to a more moderate position.

ROBERT A. BERNSTEIN

Texas A & M University

Robert Bernstein raises some interesting questions in his thoughtful comments on our study of the dynamics of candidate behavior and voter decision making in U.S. Senate elections. His analysis focuses on two points. The first concerns our conclusion that senatorial incumbents moderate as the election nears. The second concerns whether it is electorally rational for them to do so. On the first matter, Bernstein simply has interpreted the term *moderation* differently than we intended. However, this is of little theoretical consequence since we agree with him on the actual patterns of candidate behavior. On the second matter we argue that Bernstein has misinterpreted our probit equations. A proper test of the hypotheses implied by his extremism thesis demonstrates the soundness of our initial conclusions: moderation, not extremism, provides more votes for senatorial candidates.

Strategic Behavior: Candidates

Bernstein sees an "ongoing debate" over the nature of changes in incumbents' ideological positions over the election cycle. We do not see such a debate. We note some inconsistency in the way different authors have hypothesized about strategic shifts, and some differences in the use of terms. However, the work by Elling (1982), Thomas (1985), ourselves, and Bernstein all show the same thing: there is a tendency for incumbent senators to move away from their party's ideological pole and in the direction of the opposition as the election nears. Our purpose in the initial portion of our article was to provide a summary of overall candidate strategic behavior using a new data source, the CBS News-*New York Times* 1982 Congressional Poll. We saw this as building on the studies that have used roll-call behavior to assess strategic positioning. Our chief contribution here was to determine that challengers seem unable to undertake the same sort of positioning as incumbents. This is the important fact in our context of our argument.

Some confusion came from our use of the term *moderation.* Bernstein follows Elling's (1982) use of the term to mean movement toward the midpoint of the senatorial ideological spectrum. We used the term to mean the Downsian behavior of Democrats moving to the Right or Republicans moving to the Left to get closer to their states' median voter. For conservative Democrats and liberal Republicans such shifts are away from the middle of the senatorial spectrum. Thus, based on his conceptualization of the term, Bernstein argues we do not present evidence for strategic moderation. We feel our usage was clear in context, but we would probably use the term *strategic shifts* if we were writing the article today.[8] The important point is that once this simple terminological difference is clarified, our analysis does not suffer if this behavior is called *strategic shifts* rather than *strategic moderation.* There is no dispute on the empirical findings, and these are consistent with our theory of candidate issue strategies. Further, since

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we agree on the actual patterns of candidate behavior, Bernstein's methodological suggestions about measuring how candidates shift do not bear on our conclusions.

Strategic Behavior and Election Outcomes

Bernstein's second point is potentially much more important. He argues that our data show that movement *toward* the ideological extreme is a profitable electoral strategy: "Candidates receive no electoral advantage from moderating their positions. Instead, they gain by taking extreme positions." We argue that movement *away* from one's party extreme and toward the opposition and the state median voter is the vote-maximizing strategy. Four points bear on this rather substantial disagreement.

First, Bernstein states that we do not test the hypothesis that moderation gains votes. On the contrary, we take up more than a full page (p. 582) with this question. In the "Candidate and Economic Effects on the Outcome" section we show how our individual-level findings can be used to look at the aggregate consequences of candidate strategy. We provide data to support our conclusion that moderation, *here* in the sense of middle-of-the-road issue positions, does help candidates in the general election.

Second, most of his analysis rests on an inappropriate equation. Equation 2 has vote on the left side and only party, ideology, incumbency dummies, the candidate positions variables, and terms for the interaction of voter and candidate ideologies on the right. Equation 4, which includes the central variable of presidential evaluations, is a better specification of the process of candidate choice than Equation 2 on both theoretical grounds and in terms of a superior fit.[9] Hence, we include presidential evaluations in the analysis below.

The third, and probably the most serious, of Bernstein's errors occurs when he calculates the slope coefficients for candidate strategy. Using the coefficients from Equation 2, he omits a key component, erroneously believing that because b_4 and b_5 do not reach statistical significance, they should be treated as 0. It is important to realize that no significance test changes the best estimate of candidate effects based on Equation 2: the slope for Republican candidate ideology is $(b_4 + b_7 I)C_R$, and the slope for Democratic candidate ideology is $(b_5 + b_8 I)C_D$. Dropping b_4 and b_5 has the effect of saying that candidates' strategies are of no consequence among moderates, and this proposition provides the basis for Bernstein's extremism thesis.

Our Equation 2 does not provide an appropriate test for the hypothesis that candidate strategy has no effect among ideological moderates. However, such a test is straightforward: using the same data analyzed in our article, we select only moderates and run a probit analysis of vote on the candidates' ideological positions, (with party identification, incumbency, and Reagan approval as controls). Our expectation is that as Republicans get more conservative, they move away from the moderate voter and decrease the probability of a GOP ballot; and as Democrats get more conservative, they move toward the moderate voter, hence they also decrease the probability of a GOP ballot. Thus, the coefficients for both candidate variables should be negative. This is what we find. The probit coefficients are −.020 and −.018 for the Republican and Democratic candidates respectively, with t-values of −3.8 and −3.5. The foundation for Bernstein's argument—that among moderates "the probability of a Republican vote (or a Democratic vote) is completely unaffected by the extremism of the candidate"—is simply wrong.

Finally, there is a problem with Bern-

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Table 3. Simulated Election Outcomes in Twenty States Based on Alternative Senate-Candidate Strategies

State	Simulated Candidate Strategies: Liberal Democrats v. Moderate Republicans	Simulated Candidate Strategies: Moderate Democrats v. Conservative Republicans	Difference	Number of Cases
California	52.9	47.5	5.4	2,883
Connecticut	50.8	45.1	5.7	1,945
Maine	39.2	33.8	5.4	1,515
Michigan	36.6	31.4	5.2	823
Minnesota	49.2	43.8	5.4	1,181
Mississippi	38.0	32.6	5.4	953
Missouri	49.4	43.8	5.6	2,034
Montana	39.5	34.2	5.3	1,391
Nebraska	48.5	43.0	5.5	1,177
Nevada	43.9	38.5	5.4	1,124
New Jersey	49.6	43.9	5.7	1,997
New Mexico	50.4	44.9	5.5	1,128
New York	35.2	30.1	5.1	2,218
Ohio	37.1	32.0	5.1	931
Rhode Island	44.3	38.4	5.9	1,254
Tennessee	33.4	28.5	4.9	829
Utah	63.1	57.9	5.2	1,533
Virginia	51.9	46.4	5.5	935
West Virginia	32.1	27.4	4.7	633
Wyoming	58.5	53.0	5.5	870

Note: Percent voting Republican is estimated based on col. 1 of Table 6 (Wright and Berkman 1986) with liberal, moderate, and conservative candidate issue positions simulated at values −5, 0, and 5, respectively.

stein's interpretation of probit coefficients; and this contributes, we believe, to his conclusion. His analysis for the extremism hypothesis rests, in part, on the pattern of liberal and conservative voters being more responsive than moderates to differences in candidate issue positions.[10] However, differences in the slopes within probit do not necessarily translate into like differences in the probabilities of a Republican or Democratic vote. To illustrate, let us consider only the moderates and conservatives in a district; and let us assume for sake of convenience that they are equal in number. Finally, assume we estimate that a given shift by the Republican candidate to the Right yields a probit coefficient of +.2 for conservative voters and −.1 for moderates.

Bernstein would infer from this that more votes would be gained from conservatives than would be lost among moderates by the rightward shift. And on the basis of such logic he argues against the moderation thesis. However, the higher positive coefficient for conservatives does not necessarily mean a rightward move would have the desired electoral consequences. On the one hand, conservatives as a group are already disposed to vote for the GOP candidates, thus they may have a prior expected probit score (Z^*) of, for instance, 1.0 (which is the equivalent of a .64 probability of voting Republican). On the other hand, the average moderate may be undecided with a $Z^* = .0$ (probability of .5). Then the rightward shift by the candidate would change the Z^*s to 1.2 for conservatives and to −.1 for moderates. In probabilities, however, because of the nonlinear relationship between the Z^*s and their accompanying probabilities, this is an increase in the probability of voting

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Republican of only .023 for conservatives while the same probability changes −.04 for moderates. The rightward shift would have decreased the net proportion of all these voters expected to vote Republican, even though in terms of probit coefficients conservatives appear more responsive to candidate issue positions.

The point here is that to assess the likely effects of candidate strategy adequately we need not only to consider the coefficients from the appropriate specification but also to translate these into mean probabilities among the constituencies of interest. We did so in the article for a typical undecided voter. Here we consider the consequences of alternative candidate strategies for 20 state electorates.

Our test again uses the CBS News–*New York Times* 1982 exit-poll data sets. We simulate election outcomes by substituting different candidate issue positions in the individual-voter prediction equation. We use the predicted probit scores to obtain probabilities of a GOP vote at the individual level, and then calculate state means of these probabilities to obtain the expected proportion voting Republican in each state under each strategy set.

Two elections are simulated for each state: the first has a liberal Democrat running against a moderate (middle-of-the-road) Republican, while the second has a moderate Democrat opposed by a conservative Republican. The results are shown in Table 3. Notice that for each and every state—including such conservative states as Utah and Mississippi—the estimated GOP vote (mean estimated probability) is higher in the simulation where the Republican is the moderate. Likewise, of course, the Democrat does better in each case running as a moderate opposed by a conservative. The extremism hypothesis finds no support in these data.

In summary then, in spite of a terminological confusion, our theory and analysis hold up quite well. Candidates do behave in a manner consistent with our premise that they believe issues count in their elections. Further, we find no support for Bernstein's extremism thesis: our data show that moderates do respond to candidate strategies, and because of this, his deductions about candidates having to pay attention only to ideologues is incorrect. Further, when we look at voters in the 1982 elections, candidates running as centrists did better in our simulation than ideological extremists. This provides still further evidence for the electoral advantage of "strategic moderation" in contests for the U.S. Senate.

MICHAEL B. BERKMAN

Indiana University

Notes

Robert Bernstein thanks Harvey Tucker, Norman Luttbeg, and Cynthia Bernstein for their comments on earlier drafts of this critique.

1. The Wright and Berkman measure and the absolute-value method would give the same answer for the senators in a given party *only* if all Republicans were conservative and all Democrats liberal. As it is, the Wright-Berkman score reflects the relative balance of liberals and conservatives in a party rather than the moderation of senators from that party. For example, the strikingly low conservatism score of 1.0 that they get for the 11 Republicans running for reelection primarily reflects the fact that 6 of those 11 Republicans were liberals (Weicker, Durenberger, Danforth, Heinz, Chafee, and Stafford). (Note: the *N*s for the second column of Table 2 in Wright and Berkman are reversed.)

2. Thomas (1985) also compares third year with sixth year. Earlier than the third year would mean too much turnover in the control groups; later might understate strategic moderation if it does occur.

3. Wright and Berkman use senators from all states in their control group. That control group does not moderate as much as the same-state senators for 1982 (−5.1 for Democrats and −.5 for Republicans), thus strengthening the case for strategic moderation. However, that control group moderates more than the same-state control group for 1984 (5.0 for Democrats and 13.7 for Republicans), thus weakening the case for strategic moderation.

4. Thomas found, for a small sample, that conservative Democrats and liberal Republicans actually tended to move away from the center. The four conservative Democrats in this sample who were running for reelection did move slightly away from the center over the time period (but there were no

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conservative Democrats in the control group with which to compare them). The six liberal Republicans running for reelection moved slightly towards the center, while the one liberal Republican in the control group moved slightly away from center.

5. *C* was not significant by itself; *IC* was.

6. .054 and −.038 are maximum likelihood estimates and not regression coefficients, but that does not alter the analysis.

7. .054 *CI* cannot be read directly as a probability, but is best read as a *z*-transformation of the probability. Still, maximizing .054 *CI* will maximize the probability of voting Republican.

8. We discuss the problem of strategic moderation in clear Downsian terms: "Candidates . . . would like (if only for purposes of getting votes) to appear moderate to the general electorate. . . . Candidates cannot easily shift positions toward the median state voter in the short-run without risking both alienation of those core supporters and charges from the opposition of being two-faced and inconsistent" (p. 569). Our operationalization should have left no ambiguity: "If there is electoral pressure toward moderation in the general election, we expect Democrats running for reelection to be more moderate (i.e., conservative) than nonelection year Democratic incumbents, . . . while incumbent Republicans running will be more moderate (i.e., liberal) than nonelection year Republican senators" (p. 574).

9. Equation 2 was estimated to demonstrate the effects of strategic behavior on ideological voting. Some of the workings of voter ideology would be obscured by including presidential evaluations.

10. Bernstein's comment here does raise an interesting problem, the differential salience of candidate strategies to different voters. Ideological identifiers are shown in our analysis to be more responsive than moderates. However, this only scratches the surface of possible variability among voters. We suspect that similar differences may occur by levels of partisanship, incumbency, and voter sophistication or interest. These differences will be explored in a future paper.

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Forthcoming in June

The following articles, controversies, and research notes have been tentatively scheduled for publication in the June 1988 issue:

Alan I. Abramowitz. Explaining Senate Election Outcomes.

James Alt, Randall Calvert, and Brian D. Humes. Reputation and Hegemonic Stability: A Game Theoretic Analysis.

David Austen-Smith and Jeffrey Banks. Elections, Coalitions, and Legislative Outcomes.

Ivor Crewe and Donald D. Searing. Margaret Thatcher, Anthony Downs, and the Theory of Ideological Change.

Darell Dobbs and Robert Grafstein. Political Culture and Political Preferences. A Controversy.

James L. Gibson. Political Intolerance and Political Repression During the McCarthy Red Scare.

Bernard Grofman and Scott L. Feld. Rousseau's General Will: A Condorcetian Perspective.

Douglas A. Hibbs, Jr. and Christopher Dennis. The Politics and Economics of Income Distribution Outcomes in the United States.

Paul K. Huth. Extended Deterrence and the Outbreak of War.

David Laitin and Aaron Wildavsky. Three Theses on Culture. A Controversy.

Manus Midlarsky. Rulers and the Ruled: Patterned Inequality and the Onset of Mass Political Violence.

George A. Quattrone and Amos Tversky. Contrasting Rational and Psychological Analyses of Political Culture.

H. Mark Roelofs. Liberation Theology: The Recovery of Biblical Radicalism.

Kenneth D. Wald, Owen E. Dennis, and Samuel S. Hill. Churches as Political Communities.

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BOOK REVIEWS

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BOOK REVIEW CONTENTS

REVIEW ESSAYS

Bermeo, Nancy Gina. *The Revolution within the Revolution: Workers' Control in Rural Portugal*
Bruneau, Thomas C., and Alex MacLeod. *Politics in Contemporary Portugal: Parties and the Consolidation of Democracy*
Maxwell, Kenneth, ed. *Portugal in the 1980s: Dilemmas of Democratic Consolidation* ..... Walter C. Opello 253

Edquist, Charles. *Capitalism, Socialism, and Technology: A Comparative Study of Cuba and Jamaica*
Stephens, Evelyn Huber, and John D. Stephens. *Democratic Socialism in Jamaica: The Political Movements and Social Transformation in Dependent Capitalism*
Stone, Carl. *Class, State, and Democracy in Jamaica* ..... Carl Henry Feuer 257

POLITICAL THEORY

Abramson, Jeffrey B. *Liberation and Its Limits: The Moral and Political Thought of Freud*
Little, Graham. *Political Ensembles: A Psychosocial Approach to Politics and Leadership* ..... James M. Glass 261
Bélanger, André J. *Framework for a Political Sociology* ..... Sylvia Bashevkin 264
Biggar, Nigel, Jamie S. Scott, and William Schweiker, eds. *Cities of Gods: Faith, Politics, and Pluralism in Judaism, Christianity, and Islam* ..... Daniel J. Elazar 265
Blum, Carol. *Rousseau and the Republic of Virtue: The Language of Politics in the French Revolution*
Horowitz, Asher. *Rousseau, Nature, and History*
Riley, Patrick. *The General Will before Rousseau: The Transformation of the Divine into the Civic* ..... Christopher Kelly 267
Cole, Juan R. I., and Nikkie R. Keddie, eds. *Shi'ism and Social Protest* ..... Charles E. Butterworth 269
Elshtain, Jean Bethke, *Women and War* ..... Mona Harrington and Mary Lyndon Shanley 270
Faden, Ruth R., and Tom L. Beauchamp (with Nancy M. P. King). *A History and Theory of Informed Consent* ..... Fred M. Frohock 271
Fenn, Richard K. *The Spirit of Revolt: Anarchism and the Cult of Authority* ..... Michael T. Gibbons 273
Goldsmith, M. M. *Private Vices, Public Benefits: Bernard Mandeville's Social and Political Thought* ..... Richard F. Teichgraeber III 274
Jay, Martin. *Marxism and Totality: The Adventures of a Concept from Lukács to Habermas* ..... Fred Dallmayr 275
Johnston, David. *The Rhetoric of "Leviathan": Thomas Hobbes and the Politics of Cultural Transformation*

AMERICAN POLITICAL SCIENCE REVIEW
VOL. 82 NO. 1 MARCH 1988

LICENSED TO UNZ.ORG
ELECTRONIC REPRODUCTION PROHIBITED

Hampton, Jean. *Hobbes and the Social Contract Tradition* ..... Steven B. Smith 277
Levine, Andrew. *Arguing for Socialism: Theoretical Considerations* ..... Cary J. Nederman 279
Love, Nancy S. *Marx, Nietzsche, and Modernity* ..... Alan Woolfolk 280
Rouner, Leroy S., ed. *Civil Religion and Political Theology* ..... James E. Gilman 281

AMERICAN POLITICS

Brodkin, Evelyn. *The False Promise of Administrative Reform: Implementing Quality Control in Welfare* ..... Jack Tweedie 282
Denton, Robert E., Jr., and Dan F. Hahn. *Presidential Communication: Description and Analysis* ..... C. Don Livingston 283
Elazar, Daniel J. *Exploring Federalism* ..... Donald Smiley 284
Galderisi, Peter F., Michael S. Lyons, Randy T. Simmons, and John G. Francis, eds. *The Politics of Realignment: Party Change in the Mountain West* ..... Oliver Walter 285
Gibney, Mark. *Strangers or Friends: Principles for a New Alien Admission Policy* ..... Kitty Calavita 286
Gilboa, Eytan. *American Public Opinion toward Israel and the Arab-Israeli Conflict* ..... Michael W. Suleiman 288
Goggin, Malcolm L. *Policy Design and the Politics of Implementation: The Case of Health Care in the American States* ..... Barry G. Rabe 289
Goldfield, Michael. *The Decline of Organized Labor in the United States* ..... Paul Johnson 290
Hasin, Bernice Rothman. *Consumers, Commissions, and Congress: Law, Theory, and the Federal Trade Commission, 1968–1985* ..... William F. West 291
Kettl, Donald F. *Leadership at the Fed* ..... John T. Woolley 292
McFeeley, Neil D. *Appointment of Judges: The Johnson Presidency* ..... Deborah J. Barrow 294
Miller, Warren E., and M. Kent Jennings. *Parties in Transition: A Longitudinal Study of Party Elites and Party Supporters* ..... Everett Carll Ladd 296
Moffett, George D. III. *The Limits of Victory: Ratification of the Panama Canal Treaties* ..... Margaret E. Scranton 297
Ostrom, Vincent. *The Political Theory of the Compound Republic: Designing the American Experiment* ..... Jean Yarbrough 298
Peterson, Paul E., Barry G. Rabe, and Kenneth K. Wong. *When Federalism Works* ..... David C. Nice 300
Riley, Dennis D. *Controlling the Federal Bureaucracy* ..... Barbara S. Romzek 301
Robyn, Dorothy. *Braking the Special Interests: Tracking Deregulation and the Politics of Policy Reform* ..... Lawrence S. Rothenberg 302
Schutt, Russell K. *Organization in a Changing Environment: Unionization of Welfare Employees* ..... Evelyn Z. Brodkin 303
Schwartz, Herman, ed. *The Burger Years: Rights and Wrongs in the Supreme Court, 1969–1986* ..... Charles M. Lamb 304

LICENSED TO UNZ.ORG
ELECTRONIC REPRODUCTION PROHIBITED

Book Reviews

Spitzer, Robert J. *The Right to Life Movement and Third Party Politics* ..... Marjorie R. Hershey 305
Wald, Kenneth D. *Religion and Politics in the United States* ..... Anne Motley Hallum 306
White, Morton. *Philosophy, "The Federalist," and the Constitution* ..... Richard K. Matthews 307
Zimring, Franklin E., and Gordon Hawkins. *Capital Punishment and the American Agenda* ..... Mary Thornberry 308

COMPARATIVE POLITICS

Baynham, Simon, ed. *Military Power and Politics in Black Africa* ..... Sam C. Sarkesian 309
Braveboy-Wagner, Jacqueline A. *Interpreting the Third World: Politics, Economics, and Social Issues* ..... Harvey G. Kebschull 310
Elazar, Daniel J., *Israel: Building a New Society* ..... Michael Curtis 311
Fatton, Robert, Jr. *The Making of a Liberal Democracy: Senegal's Passive Revolution, 1975–1985* ..... Jonathan Barker 313
Grindle, Merilee S. *State and Countryside: Development Policy and Agrarian Politics in Latin America*
Sanderson, Steven E. *The Transformation of Mexican Agriculture: International Structures and the Politics of Rural Change* ..... John Bailey 314
Hayward, Fred M., ed. *Elections in Independent Africa* ..... Robert J. Mundt 316
Howe, James. *The Kuna Gathering: Contemporary Village Politics in Panama* ..... Philip D. Young 317
Krieger, Joel. *Reagan, Thatcher, and the Politics of Decline* ..... Michael Moran 319
Luebbert, Gregory M. *Comparative Democracy: Policymaking and Governing Coalitions in Europe and Israel* ..... G. Bingham Powell, Jr. 320
Malloy, James M., and Mitchell A. Seligson, eds. *Authoritarians and Democrats: Regime Transition in Latin America* ..... Roland H. Ebel 321
Markovits, Andrei S. *The Politics of West German Trade Unions: Strategies of Class and Interest Representation in Growth and Crisis* ..... Richard J. Willey 322
Maxfield, Sylvia, and Ricardo Anzaldua Montoya, eds. *Government and Private Sector in Contemporary Mexico* ..... James W. Dull 323
Mughan, Anthony, *Party and Participation in British Elections* ..... Harold D. Clarke 324
Padgett, Stephen, and Tony Burkett. *Political Parties and Elections in West Germany: The Search for a New Stability*
Wallach, H. G. Peter, and George K. Romoser, eds. *West German Politics in the Mid-Eighties: Crisis and Continuity* ..... Helmut Norpoth 325
Park, Bert Edward. *The Impact of Illness on World Leaders* ..... Thomas C. Wiegele 327

LICENSED TO UNZ.ORG
ELECTRONIC REPRODUCTION PROHIBITED

Sabetti, Filippo. *Political Authority in a Sicilian Village* ..... David I. Kertzer 328
Smith, W. Rand. *Crisis in the French Labour Movement: A Grassroots' Perspective* ..... Mark Kesselman 329
Stack, John F., Jr., ed. *The Primordial Challenge: Ethnicity in the Contemporary World* ..... Joane Nagel 330
Szajkowski, Bogdan, ed. *Marxist Local Governments in Western Europe* ..... Judith Chubb 333
Tutino, John. *From Insurrection to Revolution in Mexico: Social Bases of Agrarian Violence, 1750–1940* ..... Evelyn Hu-DeHart 334

INTERNATIONAL RELATIONS

Caporaso, James A., ed. *A Changing International Divisin of Labor* ..... David B. H. Denoon 335
Cimbala, Stephen J., ed. *Strategic War Termination* ..... Donald M. Snow 337
Clawson, Robert W., ed. *East-West Rivalry in the Third World: Security Issues and Regional Perspectives*
Whelan, Joseph G., and Michael J. Dixon. *The Soviet Union in the Third World: Threat to World Peace?* ..... Nathaniel Davis 338
Cottom, Martha L. *Foreign Policy Decision Making: The Influence of Cognition* ..... Stephen G. Walker 339
Hanrieder, Wolfram F., ed. *Technology, Strategy, and Arms Control* ..... Constance H. Cole 340
Harden, Sheila, ed. *Small Is Dangerous: Micro States in a Macro World* ..... Christopher C. Joyner 341
Jackson, Karl D., and Wiwat Mungkandi, eds. *United States–Thailand Relations*
Randolph, R. Sean. *The United States and Thailand: Alliance Dynamics, 1950–1985* ..... Frank C. Darling 343
Jervis, Robert, Richard Ned Lebow, and Janice Gross Stein. *Psychology and Deterrence* ..... Raymond Tanter 345
Raddock, David M., ed. *Assessing Corporate Political Risk* ..... Jeffrey D. Simon 346
Ramazani, R. K. *Revolutionary Iran: Challenge and Response in the Middle East* ..... Mohsen M. Milani 347
Thomas, Raju G. C. *Indian Security Policy* ..... Aaron Karp 349
Weiss, Thomas G. *Multilateral Development Diplomacy in UNCTAD* ..... William J. Long 350

POLITICAL ECONOMY

Hartlyn, Jonathan, and Samuel A. Morley, eds. *Latin American Political Economy: Financial Crisis and Political Change* ..... Charles Gillespie 351
Jentleson, Bruce W. *Pipeline Politics: The Complex Political Economy of East-West Energy Trade* ..... Michael Mastanduno 353
Shapiro, Susan P., *Wayward Capitalists: Target of the Securities and Exchange Commission* ..... Mark J. Moran 354
Streeck, Wolfgang, and Philippe C. Schmitter, eds. *Private Interest Government: Beyond Market and State* ..... John T. S. Keeler 355

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ELECTRONIC REPRODUCTION PROHIBITED

REVIEW ESSAYS

The Revolution within the Revolution: Workers' Control in Rural Portugal. By Nancy Gina Bermeo (Princeton: Princeton University Press, 1986. xix, 263p. $28.50).

Politics in Contemporary Portugal: Parties and the Consolidation of Democracy. By Thomas C. Bruneau and Alex MacLeod (Boulder, Lynne Rienner, 1986. xix, 236p. $30.00).

Portugal in the 1980s: Dilemmas of Democratic Consolidation. Edited by Kenneth Maxwell (New York: Greenwood, 1986. xiv, 254p. $39.95).

Although political scientists who study Western Europe have traditionally concentrated their efforts on Britain, France, Germany, and Italy, there has been in recent years a certain shift in focus to the smaller and newer democracies of the region. Despite this change, Portugal's new democracy has yet to receive much scholarly attention. This is understandable. Portugal's location on the extreme southwestern edge of the continent removes it from the mainstream of European events and its small size reduces its international importance. Moreover, it is overshadowed by Spain, which, because of its size and more widely known language, culture, and history has, comparatively speaking, drawn much more scholarly attention. Although understandable, this neglect of Portugal is unfortunate. Having recently made the transition from authoritarian rule to democracy, Portugal offers fertile ground for the testing of new hypotheses and the invalidating or supporting of existing ones in a large number of important research areas, such as authoritarian-regime breakdown, military intervention, democratic-regime instauration, regime consolidation and institutionalization, rural rebellion, party-system formation, constitution making, and the like.

It should be noted that although Portugal has been ignored by Europeanists, it has received some limited attention by Latin Americanists. Writing in the early 1970s, a certain group of these scholars sought to explain Portuguese political life from the perspective of the Latin American political experience. The general thrust of their work was to show that Portugal, like countries in Latin America, fell outside the range of the general frameworks of political development because its political process and institutions were fundamentally different from those in the rest of the less-developed world. At the same time, it was argued that Portugal could not be comprehended from the perspective of Western Europe's advanced industrial societies. The central organizing concept of these Latin Americanists was *corporatism*, which was seen as the key feature of Ibero-Latin American political systems and gave them their distinctive political life. This work, which eventually spawned a vast outpouring of studies of corporatism in northern European countries, was, however, not very fruitful for explaining the Portuguese case. The corporatist framework tended to distort the reality of Portugal's political life because it exaggerated the importance of structures of interest intermediation that were in fact little more than paper organizations and insignificant for the political process during the Salazar-Caetano dictatorship.

It should also be noted that Portugal received some considerable attention immediately after the overthrow of the dictatorship by the Armed Forces Movement (MFA) on 25 April 1974. This event generated an outpouring of works seeking to explain the "Revolution of the Carnations" and subsequent events. Unfortunately most of these works were produced in large part by journalists and scholars who knew very little about Portuguese history, society, and politics. Consequently, they did not have the historical and sociological perspective necessary to understand well the events that took place. Sound scholarship on the period of Portugal's transition to democracy is, therefore, extremely limited, being confined, until the publication of the above volume by Nancy Bermeo, to the papers presented at two meetings organized by the International Conference Group on Portugal and published in two volumes of proceedings:

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Contemporary Portugal: The Revolution and Its Antecedents, edited by Lawrence S. Graham and Harry M. Makler (Austin: University of Texas Press, 1979) and *In Search of Modern Portugal: The Revolution and Its Consequences*, edited by Lawrence Graham and Douglas L. Wheeler (Madison: University of Wisconsin Press, 1983). Two additional works need to be mentioned in this regard, Thomas C. Bruneau's *Politics and Nationhood: Post Revolutionary Portugal* (New York: Praeger, 1984) and my *Portugal's Political Development: A Comparative Approach* (Boulder: Westview, 1985).

The three books under review here are, therefore, welcome additions to the small body of sound scholarship that has been produced on Portugal. Unlike the corporatist literature, these works do not view Portuguese political life as unique and fundamentally different. Nor were they written by individuals lacking a historical and sociological perspective on Portugal. These books were produced by acknowledged and respected experts on Portuguese political and social life who have studied the country for an extended period of time. They generally seek to understand Portugal by considering it from within the broad tradition of comparative scholarship and in doing so have brought Portugal into the comparative research tradition on Western Europe, the tradition to which the country properly belongs by virtue of its location, history, and political and social experience.

Of these books, Nancy Bermeo's is the most theoretically ambitious. It deals with the rural upheavals that took place in southern Portugal (the Alentejo) in the wake of the collapse of the dictatorship. The book is divided into three parts. The first deals with the land occupations, the second with the management of the collective farms produced by these land seizures, and the third with Portugal's rural rebellion within the context of national-level politics. The primary goal of the volume is to take issue with two distinct but related bodies of theoretical literature based on the Portuguese experience. The first is that body of sociological thought which holds that rural proletarians are incapable of challenging property relations unless they are organized and led by outside groups, specifically radical political parties or unions. The second is that which argues that worker control does not necessarily lead to more equalitarian decision making within the production unit or to a higher level of productivity.

With respect to the first of these bodies of theory, Bermeo's evidence shows conclusively that the land seizures in the Alentejo do not support the idea that rural proletarians cannot act on their own. By carefully analyzing the way land occupations occurred, Bermeo shows that Portugal's southern rural labor force seized land not under guidance from outside radical forces as has usually been surmised but on their own initiative. She argues that these rural proletarians were impelled to act by the social and economic conditions (grossly unequal land tenure, secularization, military conscription, and seasonal labor migration) of the Alentejo. According to Bermeo, rural workers seized land when changes in Lisbon released the grid of authority that had bound fast the economic and social order of southern Portugal. She shows that the outside radical force usually considered as having instigated the land seizures, the Portuguese Communist party (PCP), did not lead the peasants onto the land as their vanguard but followed them.

In the second part of the book, Bermeo shows that worker control is associated with greater equalitarianism, more rank-and-file participation taken by the cooperatives, and a greater unit productivity. Although she finds that the cooperatives are equalitarian and grant numerous opportunities for participation in the decision-making process, the tendency of most workers is to defer to the elected managerial hierarchy. This, she argues, is not due to the lack of interest but to structural constraints such as illiteracy and the lack of organizational skills and, therefore, of confidence among collective-farm workers. Bermeo also finds that worker control is clearly associated with increased levels of political participation and activism in those regions of the Alentejo where they are most in evidence.

In the third part of the book, Bermeo does a masterful job of showing how politics took precedence over class conflict after the collapse of the dictatorship and how this influenced the outcome of the land seizures. She shows that the rivalry between the PCP and the Socialist party (PS) for control of the political situation at the national level led the PS to turn *against* the land seizures and cooperatives, and the PCP *towards* them. In order to defeat the PCP,

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the PS collaborated with the Center and Right to dismantle the collectives, which gradually became a bastion of PCP support outside of the industrial zones of Lisbon, especially after 25 November 1975 when the Communists lost all hope of gaining control of the state apparatus.

The most interesting and theoretically most important aspect of the book is the data that Bermeo has provided on the attitudes of rural proletarians, based on in-depth interviews conducted with over one hundred such individuals. These interviews reveal that the rural workers in the Alentejo seized land for instrumental reasons; that is, they occupied land to protect their livelihoods in the face of uncertainty created by the collapse of the dictatorship. Most of those interviewed reported that they wanted to work unused land and wanted to pay rent for its use. These responses give rise to a theoretical question bearing directly on the author's thesis; that is, does the seizure of land by rural proletarians who want to work unused or underutilized land and *pay rent* for its use constitute a challenge to the basic property-owning relations of the region? If it does, then the author's thesis that rural workers can take independent *radical* action is supported. If it does not, then the question of how rural proletarians who have acted independently but for instrumental reasons eventually produced a radical form of agricultural organization, the collective, needs more discussion. Bermeo, who recognized this problem, argues that Portugal's rural workers were radicalized by forces inherent in the "history" and "sociology" of the Alentejo. Could it not be, however, that outside forces played some role in this process, either individual workers who had been radicalized during their personal migration to Lisbon or the PCP as it followed the workers onto the land? On this point, more research is needed, which Bermeo recognizes.

The book by Thomas Bruneau and Alex MacLeod is a study of the political system installed in Portugal after 1974. The authors argue that Portugal does not need to be treated as a unique case (if ever it needed to be) and can be analyzed using "concepts" and "categories" common to the study of other West European democracies. The general purpose of the book is to show how Portugal's four major political parties, the PCP, PS, the Social Democrats (PSD) and the Christian Democrats (CDS), not unlike parties in other West European successor democracies, have captured and divided among themselves the body politic. According to Bruneau and MacLeod, this domination of political life by the parties has been very detrimental to democracy because it has politicized every nook and cranny of the Portuguese political system and has brought on severe *imobilidade* and extreme governmental instability.

The book can be divided into two parts. The first describes the emergent party system as a whole and each of the major parties separately. A wealth of useful information is presented on the organization, electoral strategy, bases of support, degree of grass-roots penetration, leadership quarrels, and factionalism in each of the parties. This part of the book also contains a discussion on Portuguese interest groups, which are found to be few in number and not very important for the political struggle because any influence they do have is mediated by the political parties through whom they must act.

The second part of the book discusses the negative consequences of excessive party domination. A chapter on the 1976 constitution and revisions made to it in 1982 shows that this document can be seen as a temporary truce among rival political parties who need to coexist within the same political system and not a fundamental agreement among them on the political and economic framework for the country. A chapter on the parliament shows how seats in that body do not represent constituents but are, in effect, at the disposal of the parties who won them. A chapter on the media shows how the state-owned broadcasting network has been colonized by the parties. Finally, a chapter on nationalized enterprises, a very large sector of the Portuguese economy, demonstrates how the appointment of managers and directors has more to do with party loyalty than technical expertise.

The image of the Portuguese political system conveyed by Bruneau and MacLeod, then, is that of political fragmentation and stagnation owing to excessive politicization. While the authors have done a superb job of describing this overpoliticization, they do not offer the reader much in the way of explanation. Here and there the book contains references to other West European political systems that have experienced similar political fragmentation and

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tantalizing references to degrees of "institutionalization" and the "personalization of politics," but there is no sustained systematic discussion of these concepts or cases and how they can help explain the present state of Portuguese political life. Despite this shortcoming, this is by far the best general treatment of the current political system in Portugal that has been published to date.

The third book, edited by Kenneth Maxwell, brings together the writing of a group of experts on Portugal that includes a former Portuguese prime minister and a former minister of industry, as well as Portuguese and non-Portuguese academics and journalists who analyze the current political, economic, and social situation in Portugal. Maxwell introduces the book with a brief but thorough survey of events since the collapse of the dictatorship a decade ago. This is followed by a journalistic account of daily life in contemporary Portugal by Diane Smith, a long-time resident who reports for the London *Financial Times*. These two chapters set the stage for the six substantive chapters which follow.

The first three of these chapters deal with Portuguese foreign relations. In one, Rainer Eisfeld, a West German political scientist who has published widely in the West European social-science literature on post-1974 Portugal, examines the structural problems associated with Portugal's accession to the European Economic Community (EEC). Eisfeld shows that the rationale for Portuguese membership was primarily political not economic; that is, membership was a way of assisting in the consolidation of democracy. He also suggests that membership will not only change Portugal in fundamental ways but will also change the EEC itself because Portuguese membership, along with that of Spain and Greece, has expanded the size of the community's less developed regions considerably, while its organizational structure, originally designed for advanced industrial countries, has remained the same. Bernardo Futscher Pereira, a doctoral candidate at Columbia, analyzes Portugal's relations with Spain. He shows how ancient Portuguese antipathy toward and distrust of Spain have impeded harmonious relations between these two Iberian nations in various policy areas: Spain's application for membership in NATO, trade relations, and fishing rights. António de Figueiredo, a distinguished Portuguese journalist, analyzes Portugal's relations with Africa and explains why the ties broken by the withdrawal from Angola, Mozambique, and Guinea (Bissau) have been retied with such rapidity. These three pieces are an important addition to the literature on Portugal because so little has been written about the country's foreign relations.

The second three of the six substantive chapters deal with domestic problems. João Cravinho, a former minister of industry, surveys Portugal's present economic crisis. He argues that the crisis is not due to temporary, short-term or misguided government policies but is caused by deeply rooted structural aspects of Portugal's economic organization. Governmental instability aggravates the crisis because it inhibits the development and implementation of a coherent long-range plan for economic modernization.

The chapter on Portuguese society by Boaventura de Sousa Santos, a legal sociologist, answers the question implicit in Cravinho's chapter: If the Portuguese economy is in such a bad state, with no hope for improvement in the immediate future, why has there not been a greater manifestation of discontent among the Portuguese population? In other words, why has life proceeded with such normality in the face of such obvious economic deterioration? According to Cravinho, the effects of the crisis in the economy are mitigated within Portuguese society by the actions of certain "compensatory mechanisms," some historical and some of relatively recent vintage, having risen since 1974. The most important of these compensatory mechanisms is the agricultural system found in northern Portugal. In that region, where live about 60% of the population, countless small family-owned farms supplement industrial wages and provide alternative food supplies. Other mechanisms identified by Sousa Santos are the "ruralization" of cities (the coming to the fore of rural habits temporarily suppressed by recently arrived city dwellers), a nationwide rent freeze (presently being phased out), and a vibrant underground economy. The author points out that while some compensatory mechanisms, such as small farms, may help the population weather the economic crisis, they ironically contribute to the maintenance of the structural problems pointed out by Cravinho inherent in Portugal's economy

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and make modernization an even more difficult task.

In the final chapter, Pinto Balsemão, a former prime minister, examines the factors that have led to a high degree of political instability in Portugal since 1974 (10 governments since 1976, not one of which has survived more than two years). Balsemão focuses on the constitution; the electoral law, which specifies proportional representation; and the semipresidential nature of the regime. Like Bruneau and MacLeod he sees the constitution as the embodiment of a temporary political truce among competing political parties and not a practical attempt to solve differences democratically. This has produced in Portugal what Balsemão calls the "institutionalization of the temporary," which makes it impossible to generate long-term coherent policies for solving problems and undermines the authority of the state. He argues that reforming the system will be very difficult because of "situationism," which refers to the control of Portuguese politics by a small political class and their clientele, who would lose power and privilege if fundamental changes were made. Maxwell's book is the best overview of the foreign and domestic policy problems faced by Portugal today. It is a must for scholars and policymakers interested in Iberia generally and Portugal specifically.

A general conclusion that can be drawn from these three books is that the political system that emerged in Portugal in the wake of the collapse of the dictatorship is the result of a temporary truce among fiercely hostile political-party elites who share a commitment to democracy at the ideological level but do not see it as practical means of resolving differences. This lack of practical commitment has generated a very high degree of political instability and aggravated economic and social problems during the last decade and will continue to do so for the foreseeable future. Stable democracy will only emerge in Portugal when its political class honestly faces the issues that divide it and devises effective democratic procedures for resolving them. This, of course, is not a situation unique to Portugal or the West European political experience. It might be useful to examine the experience of other West European successor democracies that have either escaped this kind of politicization or learned to live with it and even prospered. If this were done, our understanding of Portugal's future as a democracy would be greatly enhanced.

WALTER C. OPELLO, JR.

State University of New York, Oswego

Capitalism, Socialism, and Technology: A Comparative Study of Cuba and Jamaica. By Charles Edquist (London: Zed Books, 1985. 196p. $27.25 cloth; $6.95 paper).

Democratic Socialism in Jamaica: The Political Movement and Social Transformation in Dependent Capitalism. By Evelyne Huber Stephens and John D. Stephens (Princeton, NJ: Princeton University Press, 1986. 480p. $55.00 cloth; $14.50 paper).

Class, State, and Democracy in Jamaica. By Carl Stone (Westport, CT: Praeger, 1986. 175p. $32.05).

The failure of the development process in Jamaica, as throughout the Third World, has resulted in the social and economic marginalization of large numbers of people. An unemployment rate hovering around 30%, a level of production that in per capita terms is only three-quarters what it was 15 years ago, and a huge foreign debt are only a few examples of what this failure means for the Jamaican people.

This ongoing crisis—how to understand it and what to do about it—is the thread that links the three books under review here. Each provides important theoretical insights about the political economy of underdevelopment and makes a significant contribution to the literature on Jamaica as well. Evelyne Huber Stephens and John D. Stephens look to the Jamaican case to explore the possibilities of a transition to socialism that both truly transforms social and economic relations and deepens democracy. Charles Edquist focuses on one sector, the sugar industry, identifying the causes and consequences of Jamaica's failure to modernize sugarcane field operations. But he contrasts this to the more positive experience in Cuba and in so doing raises an important set of theoretical issues bearing directly on development experiences in the Third World. For Carl Stone, the issue is not so much change as stability; he analyzes the conditions that have allowed Jamaicans, in contrast to many people in the Third World, the relative luxury of a

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strong and stable liberal democratic order.

The Stephenses focus on the important 1972–1980 period in Jamaica. During this time Michael Manley and his People's National Party (PNP) government adopted a democratic-socialist ideology and attempted to transform Jamaican reality along those lines. While this effort did produce certain fundamental changes, it also was associated with a sharp deterioration of social and economic conditions and the ultimate rout of the PNP in the general elections in 1980. The Stephenses' analysis uses this experience not only to provide what will become the standard reference for any discussion of this important case but also to develop perhaps the most incisive analysis to date of the nature of a democratic-socialist development path in the Third World.

One key conclusion that emerges is that this generally negative experience *does not* demonstrate the nonviability of the democratic-socialist path. Rather, it serves to highlight the serious policy errors made by the Manley regime. These strategic and tactical errors, for the most part committed before 1976, effectively limited the regime's options in the ensuing period and initiated an irreversible process of political and economic decline.

The possibility of carrying out an effective political strategy ensuring a democratic transition to socialism, the Stephenses argue, foundered on two important areas of policy failure. First, the 1976 foreign-exchange crisis would have been less severe, and overall economic performance much improved if the regime had not squandered its scarce resources, especially those generated by the tax levied on bauxite producers in 1974, on distributionist measures. Rather, the emphasis should have been on productive state-sector investment projects and strong program implementation. This, along with other modifications in PNP policy, would have reduced interclass hostility, reduced capital flight, and retained key professional and managerial resources within the country. They also argue that the Manley regime should have kept a greater distance vis-à-vis Cuba and engaged in less antiimperialist, anticapitalist rhetoric. Both areas of policy error contributed to a failure to fulfill a crucial strategic component of the democratic-socialist model, the need to construct a political compromise or entente between segments of the capitalist and managerial classes and the democratic-socialist class alliance (working class, peasantry, lower middle class, and urban marginals).

Though they emphasize these contingent issues of policy choice, the Stephenses do not ignore the structural constraints imposed by dependent capitalism on any country seeking to transcend its clutches. Indeed, as they show, many of these factors, such as the chronic balance-of-payments problem and excessive import dependence and export concentration that the Manley regime inherited, were especially formidable in the Jamaican case. Nor do they ignore other historical and country-specific constraints, such as Jamaica's geopolitical importance to U.S. imperial interests or the dominance within Jamaica of imported patterns and values. They just do not see these as determinative in this instance.

Here the Stephenses enter onto difficult theoretical terrain. On one hand, they argue that the democratic-socialist model requires some accommodation with segments of the capitalist class ensuring at least toleration of the program. On the other hand, they claim that it also required a process of "social and political mobilization and ideological definition" (p. 299). There is only a fine line, however, between building a class-based socialist movement and not overly antagonizing elements of foreign and local capital.

This fine line means that achieving both strategic objectives is exceedingly difficult. The difficulty—and the tendency for an accommodationist approach to end up as a process of co-optation of the masses—is magnified, given the concrete realities of the transition. In Jamaica these included media largely controlled by capital, close ties between foreign and local capital, and a sociopolitical system open to, and heavily influenced by, metropolitan values and interests. The more conservative segments of the capitalist class had enormous resources to influence public debate and consciousness. Their sensitivity was heightened in such a resource-poor society as Jamaica, with a history of severe class divisions and tensions. In such a situation, anything that empowered and mobilized peasants and workers was likely to be perceived as anticapitalist and threatening. In this situation the Democratic Socialist party, committed to a political compromise with segments of capital, was at a clear disadvantage.

The likely outcome of this situation is a set

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of policies that might have a better economic result than those actually adopted in the Jamaican case, but without the same class orientation. Let me illustrate with an example. Between 1975 and 1977, the three largest sugar estates in Jamaica were transformed into 23 sugar-worker cooperatives. This was a controversial decision at the time, opposed by many both within and outside the administration. This decision and the subsequent difficulty experienced by the cooperatives became a key propaganda issue used against the Manley regime. It is likely that a democratic socialist regime more sensitive to economic performance and accommodationist goals, as the Stephenses suggest, would not have moved so rapidly to establish cooperatives. A state farm approach, favored by many at the time, was the likely alternative. What would have been the class orientation of such a decision? From the sugarworkers' point of view, the social relations of production resulting from a state enterprise, without a workable model of worker participation, would not have differed significantly from the old plantation model. Second, the economic decisions of such a bureaucratic entity, given the very difficult socioeconomic realities of the estates, would almost certainly have been much harsher for the workers than under cooperatives. The end result would have been more akin to the situation following nationalization of the sugar estates in Guyana: far less empowerment for the workers and far more empowerment for state bureaucrats.

A second major difficulty with the Stephenses analysis revolves around the agricultural sector. Despite their recognition that "a (if not *the*) key role in reducing dependence certainly has to be played by agriculture" (p. 274), neither their theoretical nor their empirical discussions do justice to the agrarian question in the transition to socialism. A crucial issue here is the importance, and possibility, of modernizing the export sector. (I will come back to this shortly in my discussion of the Edquist book.) Equally important is the need to eliminate rural poverty and rural-urban migration on food imports. In a situation such as Jamaica's, with a fundamental problem being the enormously inequitable distribution of resources, primarily land, in the rural parts, it is inconceivable that there can be a meaningful move to solving these problems without land reform. While there can be no gainsaying the political, social, and economic difficulties of such a path, there is no other way. Although mentioning the crucial role of the agricultural sector, the Stephenses do not adequately develop this point. Partly for this reason, they gloss over the meager efforts of the PNP in this regard. The "land reform" that occurred during this period (except in the case of the cooperatives, which were largely forced on the government) hardly deserves any such label.

It is important to consider this further. Any meaningful effort at land reform in the Jamaican context, any effort to redistribute power in the rural areas significantly, must contest terrain controlled by the foreign and local bourgeoisie. The local rural bourgeoisie, moreover, are heavily intertwined with urban elites. As Stone shows, the old rural oligarchy has been largely replaced—much of the locally owned estates are held by urban entrepreneurs or rural elites with modern orientations, many tied to the PNP itself. In other words, a meaningful effort at land reform would almost certainly do much to disrupt the entente between capitalist elements and the democratic-socialist class coalition. But a failure to carry forward such an agrarian strategy risks making a mockery of any socialist transition or the effort to improve socioeconomic conditions for the masses in Jamaica.

Charles Edquist does not focus on land reform in his book but he does emphasize the importance of agricultural modernization. The land reform that occurred in Cuba between 1959 and 1963 was a key condition for the technological transformation of its sugar industry. This sort of technical change is fundamental to the development process: "If modern techniques are not introduced in Third World countries, these countries will continue to be left behind and the vicious circle of underdevelopment will continue to operate" (p. 164). What are the conditions that facilitate the introduction of modern techniques and what are the socioeconomic consequences of technological choice? In answering these questions, Edquist develops a theory of technological innovation and compares the differential response of capitalist Jamaica and socialist Cuba to technological opportunities in the harvesting of sugarcane. His theoretically informed analysis makes this an illuminating study of why and how development does or

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does not occur in the Third World. It is also the best discussion to date of the modernization of Cuban agriculture.

Cuba's is a successful story of technological modernization. By 1985, almost two-thirds of the sugarcane crop was reaped by machine. The positive consequences of this achievement cannot be overemphasized. Harvest mechanization resulted in

1. the transference to thousands of workers of the skills to operate and repair complex farm machinery
2. the freeing up of the labor power of around 200 thousand Cuban workers
3. the establishment of a nationwide system for the maintenance and repair of harvesters
4. the acquisition by many Cuban technicians of valuable design and manufacturing experience
5. the development of a new industry, with the construction in 1977 of the largest cane-harvester manufacturing plant in the world.

In Jamaica, however, the capitalist social relations of production precluded efforts to mechanize the cutting process. Why? First, a situation of high unemployment greatly increased the political and social costs of any effort to mechanize the harvest. Second, the parameters of decision making in a capitalist economy also constrained technological change. The developmental fruits of the mechanization process can take decades to ripen; significant costs need to be borne in the interim. In Cuba, for example, it took about 15 years before harvesting machinery was really operational. Could a capitalist Jamaica undertake such an investment? In Jamaica such investment decisions were characteristically undertaken by firms, based on cost-benefit ratios calculated from the firm's point of view only. These firms, moreover, characteristically adopted only a short-term perspective, but the benefits of mechanization tend to accrue in the long run. Further, in the vertically integrated operations characteristic of Jamaica, a high degree of the value added in the production of sugar occurred in the processing and marketing of sugar, not in the farm production of cane. Hence there were fewer incentives to undertake expensive investments in mechanizing the cane harvest.

Third, the Jamaican state, at least until the 1970s, exhibited little interest and had few resources with which to initiate or coordinate the extensive effort needed to mechanize harvesting operations. In the 1970s, as part of its democratic-socialist program, the PNP *did* greatly expand the government's role in the sugar industry. But a labor-displacing mechanization policy was largely precluded by its close ties to one of the major trade unions and its dependence on workers for political support. This issue, however—the role of a democratic-socialist regime in fomenting a process of agrarian change and modernization—is not developed well in either the Stephens or Edquist books. It is a crucial area for further analysis.

If the Manley regime neither succeeded in its ambitious goals of social transformation in Jamaica, nor initiated a meaningful process of agrarian development, one important result did ensue. Jamaica's democratic political order weathered enormous political, social, and economic turbulence without significant damage. Jamaica, Carl Stone argues in *Class, State, and Democracy in Jamaica*, is a rarity in the Third World: socioeconomic instability and dependence are combined with a stable liberal democracy. Moreover, according to Stone, it is durable: "the flexibility and adaptability that have evolved . . . suggest that the democratic trends are likely to grow rather than diminish in Jamaica in the future" (p. 191). Alas, the conditions of socioeconomic deprivation for the masses also appear to be durable. The liberal democratic order appears incapable of ameliorating the conditions of a majority of Jamaicans, those very Jamaicans whose acquiescence to the political order is a condition of its stability. I foresee major tensions and disorder in the future of Jamaican democracy, rather than the growth that Stone predicts.

What, according to Stone, is the basis of Jamaica's "political success"—its development of a bourgeois political system in the face of a class system that appears inhospitable towards it and of a high capacity for managing the "certain element of turbulence" that rocks this political status quo from time to time? First, the gradual and consensual process of decolonization took place at a time of striking economic growth in Jamaica. This growth, moreover, effectively established a new, urban, and entrepreneurial elite as the dominant element

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in the economy. But, according to Stone, the economically privileged strata remained relatively detached from the main source of political power in Jamaica—the two political parties and the "outstanding" group of middle-class professionals who control them. This political ascendance of a stratum influenced but not controlled by the economic elite has allowed it to balance class conflict in Jamaica.

So the relatively autonomous political parties and the political system they control have played an effective role in "accommodating" and "reflecting" the interest of the masses; the latter in turn have tied their fate to the parties, largely eschewing other mechanisms of political expression. This exchange—political support for some welfare benefits—is nothing other than a system of political clientage. While recognizing some of the "weaknesses and deficiencies" in such a system of political representation, Stone appears surprisingly unconcerned about its manipulative nature.

While Stone is correct in arguing that there have been changes in the racist and oppressive plantation system that dominated Jamaica up to the 1940s, surely he exaggerates the transformation when he heaps accolades on the "highly competitive social system" that has developed. This is a society long identified as having one of the most skewed patterns of income distribution in the world.

It is also surprising that this political sociology of democracy in a Third World country closely linked to U.S. economic and political interests could effectively ignore a number of key actors. Multinational capital and U.S. political elites have important interests in Jamaica; their penchant for manipulating democratic processes in dependent Third World countries is well known. The reader will not find these issues discussed or even recognized here. The actual and potential role of the Jamaican military-police establishment is also neglected in Stone's analysis. Yet defense now claims a share of the government's operating budget close to twice what it was in the early 1970s, with the absolute amount growing almost 20-fold in this period. Stone's discussion would have been greatly enriched if he had attempted to confront, rather than ignore, these issues.

The need to transcend dependent capitalism in the Third World and the difficulties involved in doing this, the implications of the Cuban agricultural development model for other countries, and the conditions for, and implications of, liberal democracy are all fundamental problems of empirical and theoretical importance. These three books share a concern for, and an awareness of, these issues. Both Caribbeanists and non-Caribbeanists interested in development or comparative politics will find them stimulating and insightful.

CARL HENRY FEUER

State University of New York, Cortland

POLITICAL THEORY

Liberation and Its Limits: The Moral and Political Thought of Freud. By Jeffrey B. Abramson (Boston: Beacon Press, 1986. xi, 160p. $14.95 cloth; $7.95 paper).

Political Ensembles: A Psychosocial Approach to Politics and Leadership. By Graham Little (New York: Oxford University Press, 1985. viii, 223p. $29.95).

It is refreshing to see two books whose methodological perspective begins from the utility of psychoanalysis in the study of politics and political life. Both Abramson and Little argue that concepts like *the unconscious, repression, self, Eros,* and *psychosexuality,* can be useful in delineating the structure of political life. Both agree the study of politics requires a perspective that takes seriously the depth character of the self, the fact that unconscious structure and process affect the action and values of politics. Psychoanalysis as a way of looking at the world moves political analysis beyond its concern with the rational, the formal, the statistical and behavioral.

Little seeks a broad theory of interpretation that relies on both sociological and psycho-

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analytic insights to explain political phenomena. Abramson focuses directly on Freud's clinical and philosophical formulations, specifically the concept of eros and the relationship between eros and liberation.

Both projects are ambitious. Little devises a theory of political form or "ideal" types; what he calls *Structure, Group,* and *Ensemble.* He grounds these types in psychological category: thus, Structure brings with it self-*versus*-other relations, the strong leader, a more authoritarian or conservative kind of politics. Group encourages an emphasis on solidarity, self-*in*-other, less focus on private interest and individual will and rights. The Group leader shows more sensitivity to what the group is, what it thinks; he or she emphasizes collective goals and aims. The real-world equivalent would be leftist or socialist politics. Ensemble describes self-*and*-other, with a form of leadership Little calls "inspiring," where issues, personality, relationship, and progress define the dialogue between leaders and followers. The Ensemble leader is more of a participatory type, with an optimism about the resolution of issues and a sense of the future that brings adventure, change, possibility, and transformation. (Little for example sees Gary Hart as an Ensemble leader, although the Ensemble coalition, because of its hopefulness, may find itself brought to disillusionment and despair.)

Psychoanalytic theory helps to elaborate and draw out these political formations and types of leadership. For Structure, Freud's drive theory and his view of paternal authority, renunciation, and guilt may be considered the appropriate model. For Group, effective interpretation derives from the British (psychoanalytic) object relations theorists: Melanie Klein, Wilfred Bion, and D. W. Winnicott. And for the Ensemble relation, a participatory politics of cooperation, friendship and "play," in the sense of the spontaneous distribution of political energies, Little turns primarily to the psychoanalytic thought of Heinz Kohut and the sociological theory of Victor Turner.

Several assumptions guide both works: psychoanalysis presupposes a certain skepticism regarding human action and belief; it embraces the demystification of illusion; it presumes the embeddedness of the human self in conflict and desire. It supports change and the liberation of the self from internal and external constraint, although as a theory it is ambivalent on what liberation or restraint signifies and involves. Psychoanalytic insight is more than the dreary and pessimistic conclusions of *Civilization and Its Discontents* or *Totem and Taboo.* Nor does Freud or modern psychoanalysis necessarily support a rigid view of psychic determinism, either as clinical or political analysis. Both Little and Abramson see psychoanalysis as an interpretive method that looks for and encourages transformation in the individual and the polity, that broadens and humanizes the horizon of what politics means as a collective action. Yet they argue that the unconscious possesses a tremendous power and significance. Its presence should not be overlooked or underestimated, since to be human is to be more than a "rational actor." Little observes, "The freest man is also the most vulnerable to the unconscious, the most enlightened, to the darkest forces. This is Freud's respectful reply to Nietzsche" (p. 176).

Both arguments take issue with the Hobbesian notion that "nature" and "human nature" precede the self into the social world. Rather, the internal (the "intrapsychic") and the external (interpersonal) operate dialectically; "the individual is neither wholly himself nor wholly the construction of other people or society" (Little, p. 167). Unconscious fantasy, sexuality, longing, concern, envy, rivalry, hate: what occurs and transpires in the realm of *desire* frames the relation between self and politics: "Desire and rivalry boil under the cool hand of structure. Envy jostles equality and brotherly love in the Group. Hope and exquisite sociability waver on the abyss of narcissism, delusion and despair in the Ensemble" (p. 66)—an interesting observation of Ensemble in the context of the unraveling of the Hart campaign.

Eros in Abramson's view binds communities; in this respect, it is a creative, life-sustaining dynamic: "Much of what Freud notably taught about health and happiness depends on his appreciation of this link between human eros and human sociability" (p. xi). In Little's terms, this concept of Eros would probably fall under the Ensemble political type and the "inspiring" leader.

Yet Eros, Abramson argues, may become, particularly in groups, a form of oppression. He describes impediments to a politically expansive Eros and criticizes the Hobbesian

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notion of freedom as the "absence of opposition; by opposition I mean external impediments" (p. 133). Eros implies neither license nor the view that the self is "free" in some hypothetical state of nature where the pursuit of desire characterizes human exchange. Like Little, Abramson argues against a Hobbesian pessimism regarding the impact of "human nature" on the collective. But politics, even though Eros implies restraint and responsibility, also stimulates unconscious dynamics; and this can be a deadly component: "To what form of politics or community could one pledge rational allegiance once the need of the infant within the citizen for consolation and proxy father figures had been exposed?" (p. 6). Abramson asks an important question, because politics enacting infantile rage on the collective level may turn to jingoism, the demand for scapegoats, massive paranoia, the creation of "rational" structure for the purposes of mass destruction. (The political implications of Melanie Klein's psychological theories could be read in these terms.)

Psychoanalysis, in Abramson's view, teaches; it is a profound method of understanding; it reveals how politics "counts on an attraction and obedience that is erotic and nonrational in nature, a redirection of emotion previously reserved for the family towards the state" (p. 11). Freud refers to the "libidinal constitution" of groups (p. 11). Wilfred Bion, who receives considerable attention from Little, speaks of driving emotional constellations in groups (the "basic assumptions" of dependency, fight-flight, and pairing) that operate unconsciously and possess the power to inflict enormous damage on conscious, work-oriented tasks. Yet Abramson finds himself drawn (through Marcuse) to a Freud who demonstrates an "optimism" regarding the relation between "politics and Eros" (although considerable evidence points to Freud's dreary conclusion about the possibility of human, political cooperation).

Persistently Abramson returns in his argument to the sense of possibility and transformation in Freud's view of self and politics. Further, to be free in a psychoanalytic sense is not the popularized view of an untrammeled "self expression . . . or sexual liberation or [Wilhelm] Reichian living out of repressed desire. It lies in the slow and gradual process of self understanding which alone gives access and mastery over primitive experience" (p. 16). It is precisely this sense of self, refracted in the language and gaze of the other, struggling with its own internal resistances and forgotten pasts, that lays the foundation for a tolerant and empathic politics: "A politics of Eros will trigger its share of disputes over the distribution of Eros between individual and society. But we shall see that Freud himself was far from considering this tension to be tragic. It admits of eventual accommodation" (p. 14).

Abramson, like Marcuse, takes the side of Eros; Little, drawing from Kohut, takes the side of empathy. For each the binding, healing aspects of a public life triumphs over the more ominous antagonisms (between individual and culture) that Freud draws out in *Civilization and Its Discontents* and *The Future of an Illusion*.

Finally, both Abramson and Little see friendship as central to what politics means as an activity, a form of life, a way to resolve the "primal ambivalence in human nature between Eros and death" (Abramson, p. 15). Friendship opposes the intolerant Eros, the frenzied, "tyrannical vitality of primal hordes," the perversion of desire. Abramson looks toward a politics that transforms Eros into cohesive friendships: "At best, politics aspires to awaken the Eros of the political animal, through the experience of friendship" (p. 129). (Little adapts the Kohutian notion of a cohesive self to the larger collective and its action.) It is however a continual battle: civilization demands renunciation; it controls through guilt and the superego; it restrains; it induces aggression; it foments psychic pain; it destroys through cruelty and sadomasochistic reaction formations. No matter where it appears, Eros confronts aggression and destruction: "How Eros asserts itself against aggression and with what costs comprises the heart of Freud's political psychology" (Abramson, p. 15).

To conclude: Abramson's view of citizenship resembles Little's concept of the Ensemble relation (self-*and*-other). Each affirms political life by arguing that the movement from "friendship to citizenship" (Abramson) and "the matching of projects" between leaders and followers in an environment of mutuality and tolerance (Little) requires a commitment to individuality, limits, and respect for community—values at the heart of the psycho-

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analytic vision of human experience. Little, however, cautions against idealizing any "form" or type of politics; he argues that even the participatory dimension of Ensemble may become a victim of its own unconscious psychological conflict: "The Ensemble and ensemble relations which show us our freedom and responsibility as singular persons . . . may mislead us into a political Wonderland and the life of an eternally precocious child" (p. 177). Similarly with Structure and Group: Structure may appear as authoritarian imposition (Hobbes's Leviathan); Group may find itself suffocating and intolerant.

Both *Liberation and Its Limits* and *Political Ensembles* are lucid and well-written texts; both address the broader tradition of political philosophy and social theory (Little's comment on Weber and Simmel are masterful). Each might be understood as a critique of the mechanistic psychology of liberalism and the view that the self should be thought of as a "rational actor" or that all political choices can be reduced to their rational components. Each takes seriously Aristotle's dictum that the self is a "political animal"; and Little, drawing on Winnicott's theory of transitional objects and spaces (between inner and outer reality) emphasizes the importance of *play*—not as an irresponsible game but as the free exchange of political will and energy—in a free and participatory politics. Both interpret the relation between individual and group, and its charismatic framework—Abramson from the vantage of Freud, Little from the perspective of modern psychoanalytic theory. Abramson insightfully ranges through Freud's views on the psychology of love, cruelty, religion, sublimation, therapy, and freedom.

My only criticism of these two remarkable and highly readable books would be that each underestimates, and to some extent downplays, the dangers the psychotic process brings to politics, although Little discusses the tendency to regression in his analysis of Bion's theory of group mentality. Abramson focuses on repression, much less on *regression*. Little's Ensemble relations, Abramson's politics of Eros and friendship, the translucent citizen aware of an inner psychology and the limitations of action, appreciative of change and social otherness, may be insufficient to withstand the powerful unconscious forces of annihilation, psychotic tendencies (and pressures) that emerge from the dim underworld of the prelinguistic, infantile self. Not only may such forces, in the form of projection, projective identification, introjection, splitting, and fusion, push individuals into horrifying and incapacitating internal states of nature (Hobbes may be right in this respect), but, embodied and represented in delusion, they have repeatedly in the past hundred years brought communities, societies, and states into a boundless, mindless universe of unrestrained and hateful violence. Maybe this is reason enough to listen to the connections Freud and modern psychoanalysis draw between the intrapsychic, the interpersonal, and the group.

JAMES M. GLASS

University of Maryland

Framework for a Political Sociology. By André J. Bélanger (Toronto: University of Toronto Press, 1985. xii, 243p. $30.00 cloth; $14.95 paper).

Framework for a Political Sociology presents a clear and interesting approach to a number of important concepts in political and social research. In his study, André Bélanger sets out "to provide a framework for questioning" (p. 174), rather than to answer specific questions about social relations. He accomplishes this task by using as his starting point the notion of *social control*, defined as "a social relationship based upon a recognized social pattern . . . the social actualization of specific interests over specific resources" (pp. 15, 21). Control in turn becomes the basis for Bélanger's introduction of the concepts of *interests, conditioning, collectivity, regulation*, and finally *classes*.

In developing an approach to political sociology, *Framework* manages to link ideas and empirical findings from a wide literature in the social sciences, both North American and Western European. For example, in arguing in chapter 3 that political conditioning embraces possibilities for both pattern maintenance (e.g., socialization) and pattern formation (e.g., mobilization), Bélanger draws together such disparate yet important concepts as *political culture, ideology, social change*, and *conflict*. His illustrations of these terms are equally impressive, since they reflect a careful assess-

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ment of studies in the areas of mass media, public opinion, and education. The major strength of Bélanger's discussion, in fact, rests in its ability to organize a great deal of social theory and research in the behavioral as well as Marxist traditions and to indicate the extent to which scholarship in what are conventionally viewed as competing streams often seeks to explain similar phenomena.

On a more practical level, what is the utility of Bélanger's proposed framework? This question is addressed most directly in a concluding chapter entitled, "A Structural View of Feminism; or, What Can the Framework Tell Us about Feminism." The contemporary women's movement is mentioned initially in chapter 2, where the author suggests that social conflict follows from "a refusal to accept controls" and, in the case of the feminist movement, from "opposition to controls which currently remain in men's hands" (p. 55). This formulation is expanded at the end of the book in an effort to explain how this particular social movement takes issue with established forms of social control, notably masculine domination of Western culture as it is reinforced through traditional sex roles in the family, educational system, labor force, mass media, and so on.

From the perspective of political science, Bélanger offers a fairly innovative view of how male control of social regulation (i.e., governance) has led to two waves of feminist activism in a number of different cultures. While the forms of feminist mobilization have varied, from middle-class Protestant reformism in Anglo-American systems in the early twentieth century to socialist and Marxist variants in contemporary European states, Bélanger argues that the challenge confronting women's movements is quite similar across time and place; that is, the threat of institutionalization and deradicalization opens a crucial set of questions for researchers who "may wonder whether the movement became deradicalized because it evolved progressively toward institutionalization, or whether, on the contrary, it became institutionalized because it resigned itself to deradicalization" (p. 197). Since this type of problem is rarely posed in general discussions of politics and society, *Framework* is notable for considering women within its parameters. Readers must ask, however, whether the real-world issue here is not better identified as the resiliency of traditional control and conditioning mechanisms rather than as the strategic failures of an ongoing mobilization process (i.e., feminism).

In short, Bélanger's framework holds promise for political scientists and sociologists interested in social movements, but the starting point of the approach—social control—remains crucial in explaining how mobilization is frequently insufficient in the face of well-entrenched socialization processes.

SYLVIA BASHEVKIN

University of Toronto

Cities of Gods: Faith, Politics, and Pluralism in Judaism, Christianity, and Islam. Edited by Nigel Biggar, Jamie S. Scott, and William Schweiker (Westport, CT: Greenwood, 1986. viii, 244p. $39.95).

The revival of public concern with religion and its relationship to politics has stimulated a parallel revival of interest on the part of the academic community in the intersection between those two major forms of collective human expression. This is an abrupt change after a long period in which religion was relegated to the realm of "primordial culture," whose impact was seen as essentially residual and lessening. This new interest takes two forms: empirical studies of the relationship between religion and politics and studies of the impact of religious ideas on political life and thought. The latter, in particular has led to a revival of political theology, which today is probably an arena for more creative thinking than political philosophy.

Cities of Gods apparently tries to combine both aspects of the relationship. In doing so it produces a rather confused result. The confusion begins with its title, which suggests a certain detachment and relativism about the nature of the divinity, when in fact the only religions discussed are the three monotheistic faiths that not only share belief in the same God but insist that there is only one God as the cardinal element in their respective belief systems. Moreover, the hint of detached scholarly objectivity is not at all reflected in the book, whose authors are very much believers and very much engaged in their religious communities. This is not to suggest that the articles are

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merely subjective. Most tend to confront their traditions quite candidly, but, with one or two exceptions, they are written by committed people.

The confusions continue in part because the three religious communities discussed are so different and nothing is done by either the editors or the authors to find a common basis of comparison. Thus Judaism and Islam are correctly presented as fully political religions, that is to say, they require a shared political community for their fulfillment; while Christianity is presented as detached from the polity, though committed to trying to guide it, often from an opposition stance ("the Protestant principle"). At the same time, Judaism is wrongly presented as finding its political expression only through the Jewish state of Israel with no expectations of a Jewish political life in the Diaspora, at least not since the Emancipation. In contrast, Islam is presented as both political and ecumenical, struggling with the development of an Islamic political community for hundreds of millions of people living in many different states. Since much of this is true, it greatly complicates the problem of comparison but nothing is done to deal with these complications. If anything, the treatment of Judaism and Islam must conform to the language of Christianity, which is usually the case in such collections, distorting their own authenticity.

The best chapters in this book are those dealing with Islam. In "Islam and Political Action: Politics in the Service of Religion," Fazlur Rahman provides the best short exposition of the relationship between Islam and politics in theory and practice I have come across. The following two articles by Charles J. Adams and Marvin Zonis build on Rahman and add appropriate nuances and modifiers. Only the last chapter by Khalid Bin Sayed is more in the nature of a radical tract than political thought or analysis. It is useful as a document exemplifying contemporary Islamic radicalism. The point that all four authors make, which deserves careful attention by the West, is that even Islamic modernists are not Western liberals. They see themselves as functioning fully within the Islamic tradition, which is not a liberal tradition, even when they make a strong case that it encourages democratic republicanism.

The four articles on Judaism are correct, yet, in this reviewer's opinion, seriously flawed. Manfred Vogel is not off base when he sees "the state as essential expression of the faith of Judaism," but his discussion of the subject is so convoluted as to be of little help to a political scientist. Substantively, his error is in his use of the term *state*. The Jewish political tradition clearly rejects the notion of a reified state—until modern times there was no generic term for *state* in Hebrew. A proper polity in that tradition is the *edah*—congregation (in the original sense of the term).

Robert M. Seltzer makes liberal pluralism the touchstone of his belief system and measures Judaism in terms of whether or not it conforms, rather than analyzing Judaism on its own terms; whereas Mitchell Cohen's article, "Pluralism and Theocrats: The Conflict between Religion and State in Israel" is simply deficient. Not only does he show a lack of understanding of the many nuances and complexities in the relations between religion, state, and society in Israel, painting a highly inaccurate picture in stark black and white, but he has not troubled to familiarize himself with the by now rather abundant literature that documents and analyzes those nuances. John Pawlikowski is a sympathetic observer, as his title indicates, but he has not overcome the problem of thinking about the subject in Christian terms.

The weakest part of the book is part 2, on Christianity. Although Martin Marty's chapter is up to his usual standard, the others show all the confusions of contemporary liberal Christianity when approaching questions of religion and politics. On the one hand, they want to develop a political theology; but on the other, they do not want to acknowledge the legitimacy of the exercise of power other than under the most benign circumstances, where politics is essentially a matter of civil cooperation with no hint of the possibility of coercion. Contemporary liberal Christianity does not have the stomach to hold on when the going gets rough and this is abundantly evident in these chapters. The chapters also demonstrate what happens intellectually when there is the kind of severed connection between religion and polity which is characteristic of the contemporary West, particularly the United States, where the gap between the religious and political communities is so great that the religious community does not bear any responsibility for the

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consequences of political acts, their own or others.

The Islamic section is best perhaps because it is least influenced by Christian intellectual frameworks and terminology, trying rather to see Islam in its own context. The section on Judaism never quite overcomes the fact that the writers are all U.S. citizens involved in religious thought, used to writing in Christian language. This is a constant problem of Jewish-Christian dialogue in the United States and it is one that influences the comparative study of politics and religion as well, as this book amply illustrates. It is to be hoped that those concerned with the effort will devote substantial time to breaking through the limits of language so that truly comparative studies can be developed. In the interim, this book is a good example of where the subject now stands. That is both its strength and its weakness.

DANIEL J. ELAZAR

Bar Ilan University
Temple University

Rousseau and the Republic of Virtue: The Language of Politics in the French Revolution. By Carol Blum (Ithaca, NY: Cornell University Press, 1986. 302p. $24.95).

Rousseau, Nature, and History. By Asher Horowitz (Toronto: University of Toronto Press, 1987. 273p. $30.00).

The General Will before Rousseau: The Transformation of the Divine into the Civic. By Patrick Riley (Princeton: Princeton University Press, 1986. 274p. $27.50).

Without overtly disagreeing with each other, these three recent books give strikingly different pictures of Rousseau. The differences stem in large part from decisions about the proper perspective in which to view Rousseau: whether he should be considered in the light of an earlier tradition or in terms of those for whom he prepared the way; whether he should be read for his place in the history of political thought or for his influence on political practice. Each approach carries its own set of hazards.

Many traps await those who attempt to trace influences on thinkers. To his credit, Patrick Riley avoids the very seductive trap of turning earlier thinkers into "proto-Rousseauians" (p. x). Riley carefully explores the rich background of dispute about the *general will* in the works of Arnauld, Pascal, Malebranche, Bossuet, Fénelon, Bayle, and Montesquieu to show what this notion meant in the century before Rousseau. By doing so he reminds us that, for Rousseau and his predecessors, fundamental political problems were inseparable from theological questions.

Riley traces the origin of the term *general will* to interpretations of St. Paul's remark at 1 Timothy 2:4, "God wills that all men be saved" (pp. 4–5, 9, 68 n. 8). From this passage come questions about how anyone can be damned and whether salvation comes from one's own efforts or by divine grace alone. The general will was born in 1644 with Antoine Arnauld's claim that God's general will, which is that all *sorts* of people be saved, does not conflict with his particular selection of a few. Thus the *general will* begins as an explanation of God's justice.

Contention over the divine general will ran through the succeeding generations. The pivotal figure in this debate is Nicholas Malebranche who opposed the Jansenism of Arnauld and Pascal and was later opposed by Fénelon and Bossuet. Malebranche turned emphasis away from salvation to the orderliness of nature, which follows general laws issuing from God's will. He attempted a union of Christianity with the new Cartesian physics, in which the roles of both particular providence and miracles were reduced so much that it was objected that "Malebranche does not really offer nature *and* grace at all: he offers just nature and grace vanishes" (p. 71). Although Riley only occasionally alludes to the high political stakes involved (cf. pp. 6–7), he outlines these theological disputes with painstaking attention to detail.

Next Riley describes the "secularization" (pp. 52, 79, 101, 171) of the general will by Bayle and Montesquieu. He explains Bayle's distinction between generality and particularity in his account of human justice and refers to both Montesquieu's analysis of general natural causes that influence human society and his "conversion of the general will of God to the general will of the citizen" (p. 250). Riley does not decide whether this conversion simply uses or subverts the theological term. In describing this "secularization" he reasonably objects to excessive claims about Hobbes's importance (pp. 177–80). He observes that

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Hobbes did not use the term and that Hobbes's sovereign is a particular. Nonetheless, Hobbes does call the law "the general providence" exercised by him (*Leviathan*, chaps. 26, 30). The sovereign's will, like God's, has a general application.

Finally, Riley suggests that the notion of *generality* is the key to "the overall coherence and unity in Rousseau's thought" (p. 258) and points to elements of Malebranchism in Rousseau's fictional characters. However, he could also call attention to the importance of particularity in Rousseau's descriptions of the state of nature (where lack of any sort of general knowledge contributes to contentment), the customs necessary for healthy political orders, and his own life. Reconciling generality with a more fundamental and natural experience of particularity is the crucial issue. Rousseau does present the general will as an imitation of the "immutable decrees of the divinity" (Geneva manuscript of the *Social Contract*), but he claims that the inaccessibility and ineffectiveness of these decrees make this "imitation" necessary. Thus Rousseau's doctrine of the general will is based on a criticism of earlier versions of the doctrine. Riley's book sheds considerable light on the earlier versions against which Rousseau presented his thought.

While Riley looks at the traditional character of Rousseau's language, Asher Horowitz insists upon looking forward to a very different language. Horowitz argues that Rousseau's traditional language conceals his radical novelty and that understanding his depth and complexity requires translating him into the language of Marx and Marcuse rather than that of Malebranche (p. 86). Accordingly, he offers a "left Freudian reading" of Rousseau (p. 4). Rendering Rousseau in such new language invites the question of whether something gets lost in the translation.

Horowitz's "imaginative reconstruction" (p. 9) of Rousseau centers on the historical character of human existence. His account of history in Rousseau stresses labor that "distinguishes human beings from all other forms of being" and is "the primordial form of historical activity" (p. 31; see also pp. 73, 76). Rousseau, however, gives labor a less dominant, though still important, role. In *Emile*, Rousseau immunizes his student against the effects of division of labor, but he presents anger, religion, submissiveness to authority, and sexual desire as equal or even more fundamental dangers. Moreover, in the *Second Discourse* he identifies "perfectibility" as the distinctive human characteristic (see Horowitz, p. 73). Perfectibility manifests itself in labor, but also in fear, sexual desire, and other factors that bring humans together and separate them, thereby generating history.

From his focus on labor, Horowitz argues that Rousseau identifies at least three "distinct socio-economic forms," each based on a predominant type of labor: "primitive society, traditional or pre-capitalist society, and bourgeois or civil society properly so-called" (p. 90). These categories replace Rousseau's own of savage, barbaric, and civilized societies. Whereas Rousseau claims that nascent society is durable and that most peoples remain in this condition, Horowitz stresses its fragility in the face of technological developments (p. 99). Horowitz's interpretation has a certain validity, but it changes Rousseau's emphasis considerably. Horowitz also insists on the "relatively high degree of technological development and division of labour" in the most advanced societies (p. 114). This formulation may obscure the fact that republican Rome, Athens, Persia, and ancient Egypt all belong in this category of civilized community. Thus, although Horowitz distinguishes Rousseau from Marx on several points, he consistently blurs significant distinctions between the two in his effort to contrast Rousseau with his predecessors.

Using his framework, Horowitz gives novel interpretations of *Julie* as a doomed flight from the tensions of bourgeois society, the *Social Contract* as an ultimately self-contradictory community based on bourgeois premises, and *Emile* as a transcendence of bourgeois society. The most interesting and valuable of these is the discussion of Rousseau's reservations about the community of the *Social Contract*. Although Horowitz could show that these reservations extend to all civilized communities and explain the significance of Emile's political education; nevertheless, he succeeds in demonstrating the superiority of Emile's reconciliation of nature and civilization to the denaturing required for citizens.

Horowitz could apply his general argument about history more radically to the question of the foundations of Rousseau's knowledge. While he argues that Rousseau links philoso-

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phy to history and alludes to the *Confessions* as an example of historical method (p. 29), he dismisses the autobiographical writings as "apologetic introspection and mystical experience" (p. 166). Nevertheless, in these works Rousseau explains how he acquired his understanding of both nature and society in spite of being affected by society. Horowitz does not investigate this point, but his analysis allows the reader to pose this important question.

Unlike Horowitz and Riley, Carol Blum focuses attention on Rousseau's personality. Her book concerns "the fictional construction labeled 'Jean-Jacques Rousseau,'" not the philosopher (p. 13). Blum uses this "cultural artifact," which consists of what Rousseau's readers made of him and his books, to answer the much debated question of his influence on the French Revolution. Although she frequently warns that the main protagonists of her book, Saint-Just and Robespierre, simplified Rousseau's thought, ultimately she considers them "authentically faithful to a central core of the master's teaching" (p. 277). Blum demonstrates that an image of Rousseau cast a powerful spell on the most radical of the revolutionaries and, indeed, on their contemporaries.

Blum elaborates on Jean Starobinski's claim that while Rousseau was not the theoretician of the revolution, "the Terror seems to me to be, on the political level, homologous with what unfolds on the mental level in the autobiographical writings of Rousseau" (quoted pp. 18, 218). In Blum's elaboration, the revolutionaries' image of Jean-Jacques "combined claims of moral superiority with a covert erotic (if infantile) invitation" (p. 50; see also p. 109). Crucial to his seductive appeal was an equation of "the virtuous, the natural, and the asocial" (p. 57). This combination led those infected by it to glorify their own innocence and to condemn the outside world. Especially in Robespierre, the identification with Jean-Jacques led to an obsession with martyrdom and willingness to encourage assassination and terror. In short Blum argues that Rousseau gave the revolution its model of revolutionary psychology.

Blum presents ample evidence that people like Robespierre were powerfully affected by an image of Rousseau. Nevertheless, their (or her) equation of the virtuous, the natural, and the asocial distorts Rousseau's position. Rousseau consistently presents virtue in almost all of its senses as a political and unnatural characteristic. Accordingly, Blum might have made more of the occasions during the Revolution in which conflicting interpretations of Rousseau were set against each other. The more moderate revolutionaries may have been affected by Rousseau as much as the extremists were and at least as true to his thought.

In spite of the unsympathetic description of Rousseau's personality and inaccuracies on a few points, Blum points to an often neglected practical and theoretical issue. His activity as a novelist and composer shows that Rousseau wished to affect his audience and he certainly intended to inspire identification with himself. As Blum says, "Rousseau's life is a *problem* almost exclusively because of its literary representation by Rousseau" (p. 77). As she further points out, Rousseau used his "life" to overturn the respectability of the rich, the powerful, the intellectuals, religious authorities, and others. He presented a simplified popular image of himself as well as a complex theoretical teaching. Blum's book clarifies a part of the effect of this popular image. More about both this popular effect and Rousseau's deeper importance could be found in how readers such as leading poets of France, Germany, and England viewed Rousseau in the 50 years after his death. No doubt scholars will continue to ponder this thinker, his influence, and his effect.

CHRISTOPHER KELLY

University of Maryland, Baltimore County

Shi'ism and Social Protest. Edited by Juan R. I. Cole and Nikki R. Keddie (New Haven: Yale University Press, 1986. x, 325p. $40.00 cloth; $12.95 paper).

To begin with the positive, the introduction by Cole and Keddie provides an excellent, balanced account of the multiple forces of Shi'ism and of the factors—historical, social, economic, and political (i.e., Iranian-based inspiration or assistance)—that have prompted increased political activity and social protest among Shi'i groups throughout the Middle East. Moreover, all of the articles are informative and well documented, though not all of the same quality. Cole and Keddie are to be congratulated for finding contributors thor-

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oughly acquainted with the area, capable of using primary sources in the different language areas studied, and willing to look at their particular domain in a new light.

Here, however, is where the faults of the book become most apparent. Despite the claims by the editors that "the purpose of this volume is to help fill gaps in our knowledge about the various Shi'i communities who have recently shed quietism for political activism, from Lebanon in the west to Afghanistan in the east" (p. 2), four of the volume's 11 chapters fail to address this general theme. The four are Fred Halliday's account of Iranian foreign policy since the revolution (chap. 3), Nahid Yeganeh's and Nikki Keddie's discussion of how female sexuality has been viewed in Iran prior to and after the revolution (chap. 4), Rudi Matthee's description of changes in the way publications from Egypt's Muslim Brotherhood and Islamic Left movements viewed Iran's revolution (chap. 10), and Muriel Atkin's survey of the various postures taken by Soviet scholars in the last 25–30 years towards Shi'ism and Shi'i political activity (chap. 11). Excellent and interesting as each of these essays is in itself, none sheds light on the basic theme of the book as identified by the editors.

The seven chapters that do address this theme fall into three groups. One is represented by Richard Cottam's subtle characterization of the Iranian Islamic Republic as an authoritarian populist regime and his argument that Muhammad Reza Shah's regime became vulnerable by failing to offer any outlet for the desire among growing numbers of Iranians to participate in governmental activities (chap. 2). Another, the development of Shi'i protest in the Persian-Arab Gulf (that is, in Iraq, Saudi Arabia, and Bahrain) finds expression in R. K. Ramazani's integration of Khomeini's concept of security in the Gulf with case studies of Shi'i political activity in each of these states (chap. 1), Hanna Batatu's detailed analysis of two Shi'i groups in Iraq (chap. 7), and Jacob Goldberg's narrative about the treatment of Shi'is in Saudi Arabia (chap. 9). The third, Shi'i social protest elsewhere, is addressed with respect to Lebanon by Helena Cobban (chap. 5) and Augustus R. Norton (chap. 6) and with respect to Afghanistan by David B. Edwards (chap. 8).

Clearly, there is duplication of effort here. The articles by Cobban and Norton are so similar that one wonders why both were included or why each author was not asked to focus on different issues. Nonetheless, Cobban's article is especially important for the prognoses she offers with respect to Lebanon's political future, and Norton's for his critical analysis of how Shi'i protest has developed in Lebanon in the last 40-odd years. One also wonders why Ramazani was not urged to provide case studies of gulf states other than Iraq and Saudi Arabia and why his punctilious quibble with Batatu over the precise founding date of the Islamic Call was not excised (compare p. 41, nn. 8–10 with pp. 191–92, nn. 30–36).

Other indications of editorial laxity are the excessive variation in the length of the articles and in the terminus ad quem for research in them. More serious, however, are the conflicts between authors. Cobban and Norton, for example, offer different translations for the military movement of which Amal is the acronym (pp. 144, 167) and vastly different figures of casualties resulting from the March 1978 Israeli invasion of Lebanon (pp. 146, 168). The editors and Batatu appear to ignore each other's explanation of the difference between the Akhbari and Usuli schools within Twelver Shi'ism (pp. 7–8, 187, n. 16) and Muriel Atkin pays no heed to Norton's insistence that Sayyid Mūsa al-Sadr was known as Imam (pp. 293, 160–61). Even the map placed at the head of the volume lacks utility, insofar as it fails to locate the many lesser-known places cited by the various authors and even some of the better-known places, such as Basra.

In sum, this is a collection of excellent articles more or less addressing a similar theme. Each is interesting and important in itself, but few come together to form a coherent whole.

CHARLES E. BUTTERWORTH

University of Maryland, College Park

Women and War. By Jean Bethke Elshtain (New York: Basic Books, 1987. xvi, 288p. $19.95).

Women and War is a work that deconstructs influential narratives about war in the Western tradition in an effort to undermine falsities in their teaching—especially false dichotomies

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that limit our sense of political choice and thus by implication teach the inevitability of war. Elshtain examines works as divergent as Greek tragedy, Kantian philosophy, and popular movies from *Shane* to *Rambo*, and connects their messages to the readiness—even eagerness—for war carried in the currently dominant theory of realism, which she strongly critiques.

Elshtain divides her book into two parts. In part 1, "Armed Civic Virtue," she examines the multiplicity of images of the armed warrior as an exemplary figure in works of political theory from Homer, Plato, and Aristotle to Lenin and also examines various "just-war" theories, arguing that they are often a prop for, rather than a brake on, "armed civic virtue."

In part 2, "Life Givers/Life Takers: History's Gender Gap," Elshtain sets out to lay to rest the message of one of the "dominant discourses" about war: that women are peace loving and life preserving, while men are violent and bellicose. That discourse has perpetuated a variety of false conceptions about men and women—their natures, social roles, and political voices—that have served to reinforce realist justifications of war. Men have been taught by war narratives from the Greeks to the present to understand virtue as readiness to fight for one's nation, to accept the bearing of arms as the highest act of citizenship. The same narrative tradition teaches women that their virtue is to decry violence but nonetheless to praise male valor and courage, to support war with home-front duties, to comfort the wounded, to mourn the dead.

Mounting a series of counterexamples, a parade of bloodthirsty women and peace-loving men, Elshtain urges the separation of fact from fiction. Most importantly, she insists, we must distinguish the real from the dogma of realism that depicts war as inevitable, even "natural," when in fact it stems from the decisions of elites whose own thinking may be beclouded by false images and fantasy.

Her deconstruction of the war-peace and male-female dichotomies leads Elshtain to argue that those who oppose the destructiveness and inhumanity of war should stop proposing "peace" as the alternative to "war." The image of peace in most narratives is bucolic, devoid of conflict, apolitical, and ultimately false in its promise of harmony. By contrast, drawing on the work of Hannah Arendt, Elshtain seeks "wartime enthusiasms without war" and new narrative structures in which collective violence is not counterposed to "peace" but to "politics" (pp. 231, 237). Specifically, she proposes a politics of "chastened patriotism," whose practitioners would be urged to accept differences, understand limits, practice restraint, and know when to forgive: the tenets of a growing body of feminist theory about war.

One problem with the book is that while it examines the dynamics by which false conceptions of male and female natures encourage the recourse to war, Elshtain does not develop a larger causal theory. Why have our dominant narratives made it seem that war is inevitable? What, if any, is the relationship between patriarchy and war? Elshtain does not directly address these questions. Nonetheless, she demonstrates the power of narrative to shape political thinking and advances a compelling argument: questions about gender must be brought to the study of international relations and great-power politics to free the discipline from its conventional (and false) realist paradigms.

MONA HARRINGTON
MARY LYNDON SHANLEY

Vassar College

A History and Theory of Informed Consent. By Ruth R. Faden and Tom L. Beauchamp (with Nancy M. P. King) (New York: Oxford University Press, 1986. xv, 392p. $29.95).

To consent is to agree, to be of the same mind. Dictionaries, always misleadingly clear about what words mean, elaborate *consent* as the voluntary acceptance or allowance of what is done or planned by another. All lexical accounts classify *consent* as an act of reason or deliberation.

Faden and Beauchamp explore the history and theory of informed consent in a variety of social practices, with special emphasis on bioethical issues. The book is structured along three lines. The first is an examination of those moral and legal concepts that illuminate informed consent. The second is a delineation of a history of informed consent in selected areas

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of biomedical traditions. The third consists of the authors' theory of informed consent drawn from the lessons gleaned from philosophy, law, and history.

One of the better chapters in the book (written mainly by Nancy King) is a study of consent and the courts (chap. 4). We are provided with a helpful sketch of the mechanisms that historically introduced informed consent into medicine. The authors point out that medical malpractice is in general based on beneficence, which identifies the rules for proper medical care that protect the well-being of patients. But recent negligence cases do rest on a failure of informed consent, and are developed on the acceptance of patient autonomy. In a practice where physician and patient share the same goals, a legal concern for self-determination has "jolted" medical practitioners into accepting the rights of patients to accept or refuse therapy.

The (ironically) coercive introduction of informed consent from outside biomedical practices helps explain why research on human subjects is more nearly guided by informed consent than is clinical medicine. Put simply, the state has compelled research scientists (via regulation) to obtain informed consent from their subjects while merely urging physicians "to adopt a model of shared decisionmaking" (p. 224). But even the mandatory character of consent requirements in research is both recent (dating only to the mid-1970s) and a reaction to the well-known moral failures represented by Milgram's research at Willowbrook, Tuskegee, and the Jewish Chronic Disease Hospital. The authors remind us that biomedical practitioners have not taken the initiative in implementing informed consent.

In the context of this legal and political history the authors proceed to explain informed consent in terms of its embedded concepts. Autonomy is disentangled as a cluster of intentions, understandings, and freedom from controlling influences (p. 238). Each of these three components of autonomy is elaborated, with the usual distinctions recognized among coercive, manipulative, and persuasive influences. The authors admit that autonomy requires independence both from others and from oneself and that the latter is impossible to fix in the absence of a theory of the self.

The theory of informed consent developed in the book relies on a distinction between informed consent as autonomous authorization ($sense_1$) and legally or institutionally effective (valid) authorization from a patient or subject ($sense_2$). The former more nearly expresses a moral sense of informed consent, while the latter is represented in conventional rules that regulate those who seek consent. The authors show how these two senses of informed consent are different and sometimes contrary interpretations and that they provide a more robust view of the concept than standard definitions based on disclosure, comprehension, voluntariness, and competence. On the basis of this richer understanding of consent and relying on historical materials, the authors urge a shift from disclosure to effective communication in the practice of informed consent.

The book is a helpful and often authoritative account of informed consent that moves comfortably between empirical and philosophical approaches. The empirical material is secondary, representing no field work. But it is useful to have political and legal policies and decisions collected in narrative form. It would be even more useful to have some recent data on clinical medicine to document accurately how consent is pursued by health professionals. Missing from the book is any extensive treatment of the hard and important cases of no-code and partial-code practices in medicine. Living wills receive no mention at all. The issues of consent emerging from such cases invite us to survey some of our deeper values on life and its qualifies, and how life-and-death decisions are and ought to be made in liberal societies. One leaves the present work disappointed that the considerable skills possessed by Faden and Beauchamp were not brought to bear on consent in critical care medicine.

Also missing in the work is a sensitivity to alternative communities of discourse that might provide radically different accounts of informed consent. Religious communities, for example, map the embedded concepts in consent within ontologies different from, and hostile to, those informing medical communities. The present work touches on these issues with brief discussions of false beliefs (pp. 253–54) and the need for intrasubjective components in identifying materially relevant propositions in consent decisions (pp. 309–10). But the authors do not explore the intriguing possibilities that

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all or most of our criteria for informed consent, including that which specifies competence, are captives of competing discourses. The authors are content to elaborate informed consent within the conventions of medical and legal discourses.

One also wishes that more speculative philosophy, especially recent work on primitive terms in the discourses the authors do address, could have been introduced. The work of Parfit (and others) on individual identity might enlighten our understandings of the self assumed in theories of autonomy. Can serial selves give informed consent, for example? Does autonomy require nonreductionist selves?

The book is an important work within the traditional areas of empiricism and philosophy that increases our appetites for different and perhaps more fanciful explorations of informed consent.

FRED M. FROHOCK

Syracuse University

The Spirit of Revolt: Anarchism and the Cult of Authority. By Richard K. Fenn (Totowa, NJ: Rowman & Littlefield, 1986. vii, 179p. $27.95).

The problem of modern society, argues Richard Fenn, is that it mistakes what passes for a rationalized social order as an indication of the secularization of society. This misperception is the outgrowth of the priestly paradigm of secularization, which locates the voice of reason in a priestly class. In the modern world the priestly class consists of state and institutional authorities who seek to make institutions, especially the modern state, immune from popular and spontaneous action (p. 3). The result is that the state is identified as the ultimate seat of reason, which must control social conflict through ritualization to maintain order.

From the perspective of an alternative paradigm, what Fenn calls the prophetic paradigm, the modern state represents only a partial secularization of society. The representation of the state as the source of reason and necessary order results in fact in the "cult of authority" consisting of the self-identification of individuals with the state. It feeds on the fears, anxieties, and helplessness that people experience in increasing, manufactured complexity (p. 90) of society. It further maintains itself through the ritualization of social conflict and participation. Ritualization, Fenn argues, displaces social conflict without really resolving it (pp. 143–49). The result is a lack of conclusive action, a "sham process of manufacture from which social facts never emerge complete and finished. . . . No decision is final and no grievance finally resolved" (p. 66). The problem is not only that participation is a fraud because it is ineffective. In addition, the "illusions of security" it promotes "can lead an entire community to the brink of self-destruction" (p. 128).

As an alternative to the pathology of the modern state, Fenn claims to offer an ideal that gives shape to "the possibilities for genuine freedom and unfeigned solidarity" (p. 19). Anarchism, the "emancipation of humankind from all forms of order except those arrived at by continuous and spontaneous acts of free consent, requires that the state be replaced with more flexible, simpler, and smaller forms of collective organization" (p. 89).

Fenn recognizes that the anarchist project involves various ambiguities. For example, it may require the emergence of heroic individuals. But the flip side of the anarchist hero can be contempt for the rest of humanity (pp. 150–53) that tempts the anarchist to authoritarian action and must be resisted. Nevertheless, the direction of the modern state, its failure to resolve social conflicts, and the disaffection that ritualized social action may create make anarchism the only alternative that guarantees free, uncoerced communities of autonomous social actors.

Fenn's argument speaks to many of the concerns of contemporary political theory. His claim that the connection between personal identity of citizens and the state as a cult of authority is a challenge to those political theorists who have attempted to outline the foundations of legitimacy of the state based on a connection to autonomous personal identity. Because it speaks to many issues in contemporary political theory, several qustions remain (in addition to the traditional challenges of how anarchism is to be realized in practice). First, Fenn's discussion implies that in the modern world the overriding threat to autonomy is the state. But alternative diag-

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noses of modernity locate the inefficacy of political action in a distorted relationship between the state and the economy that is part of an increasingly interdependent economic world. Such accounts are important for they raise the possibility that constraints on autonomy are not inherent in the modern state as Fenn would have it and that perhaps the modern state can be reconstructed as the embodiment of individual and communal autonomy. Second (and connected to the first point), though it may not be a "*gross* oversimplification to argue that complexity in social life is imposed by elites to perpetuate their domination" (p. 90), it *is* an oversimplification. Whether a public sphere of small communities characterized by what Fenn calls *limited obligation and spontaneous political action* would be capable of coping with that complexity is problematic. Third, one wonders if Fenn's impatience with "manufactured" social facts and inconclusive political action is a protest against the social construction of reality and the ambiguity that many theorists argue is inherent in modern politics and a precondition for freedom and autonomy in complex societies.

Fenn's book serves as a reminder of the potential dangers of the connections between personal identity and the modern state. And in considering the anarchist ideal it may be important to keep in mind Kropotkin's claim that "mental timidity in constructing an ideal is certainly a criterion of mental timidity in practice" (quoted p. 154). But it is also important to keep in mind that political ideals that are unsustainable or unrealizable can issue in social despair and political cynicism that are the resources of authoritarian political arrangements.

MICHAEL T. GIBBONS

University of South Florida

Private Vices, Public Benefits: Bernard Mandeville's Social and Political Thought. By M. M. Goldsmith (Cambridge: Cambridge University Press, 1985. ix, 183p. $29.95).

Three of the six chapters in *Private Vices, Public Benefits* have been published already—as two somewhat shorter articles—in easily accessible journals. Those earlier articles were significant contributions to a revival of scholarly interest in Mandeville that began in the mid-1970s and has resulted in other book-length studies by Paulette Carrive, Richard Cook, Thomas Horne, and Hector Monro. There are two principal novelties in this volume. The first (in chap. 3) is an account of the *Fable of the Bees* showing that Mandeville understood the development of social institutions as the unintended consequence of human interdependence. The other (in chap. 4) is a detailed presentation of Mandeville's political views as those of a lifelong, if often "independent," Whig.

Goldsmith's chief objective in this book is "to show what Mandeville was doing in writing what he did" (p. ix). Yet readers interested in developing a better historical understanding of Mandeville's cynical, often bawdy humor will be disappointed. Indeed, that is a well-known aspect of his work Goldsmith is determined to play down. What he offers instead is a portrayal of Mandeville as a theorist whose thoroughgoing affirmation of human appetites and passions is best understood as a criticism and rejection of an ideology of "virtue" prevalent in Britain during the half century following the Glorious Revolution. According to Goldsmith, that ideology was a fusion of Christian and civic republican ideals of virtue that led to the formation of societies designed to promote Christianity and reform manners and (for Mandeville) also found its chief expression in Richard Steele's *The Tatler*. In this setting, it is clear why Mandeville's views were so scandalous for some of his contemporaries. For in defending natural human inclinations to pursue private goods and pleasures, Mandeville sought to justify what many took to be nothing more than "vice." Mandeville was also troubling because (unlike Hobbes) he acknowledged that individuals enjoyed the company of their fellows but not for reasons proponents of "virtue" could accept. Mandeville stressed that man's sociability was rooted in self-love, not natural benevolence. Men live in society because of their vanity, because they seek the flattery and admiration of others, and because they believe others are possibly aids to satisfying their material desires. Indeed, the main source of social interdependence is pride, what Mandeville often called *self-liking*. By nature men are indolent, yet they overcome their instinctive

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indolence because they are also self-seeking. To pursue "virtue" in the face of these obvious facts, Mandeville argued, would be ruinous to the progress of society. A few individuals may come to find virtue as a result of extraordinary self-restraint or self-denial; but "virtue" here is attained at the cost of other benefits, such as sociability or the common pursuit of wealth. Mandeville thought that the vast majority of men would not pay such a cost. Hence, our "public benefits" must be rooted in our "private vices," and those who would impose "virtue" on man's private passions propound dangerous nonsense.

Despite some repetitiveness in his analysis, Goldsmith's chief contribution to a fuller understanding of these views is to show how they took shape as a direct response to an ideology of virtue that informed much of British opinion after 1688. But there are problems with this approach and in Goldsmith's attempt (in chap. 5) to link Mandeville's views with "the spirit of capitalism." The main problem with Goldsmith's approach is that it still leaves one wondering how the *Fable of the Bees* ever achieved such a great *succès de scandale*. It may be true that Mandeville began his thinking within an established British ideology of "virtue," but it simply does not follow that an attempt to measure the power and virtuosity of his thinking should remain largely confined within that ideology. Such an approach leads to a neglect of other motives in Mandeville's thinking and sheds little light on the question of why Mandeville was to be both the foil and the inspiration of subsequent eighteenth century thinkers (especially David Hume and Adam Smith) whose work was at best marginally related to the concerns of the proponents of Christian and civic virtue.

Tying Mandeville to the "spirit of capitalism" is certainly one way of addressing these issues. But Mandeville's endorsement of money-making (pp. 134–37) does not make him an advocate of "capitalism." It is now widely accepted that in their understanding of eighteenth century "commerce" mercantilist thinkers shared many important assumptions in common with their supposed "free-trade" rivals. Chief among them was an analysis of human psychology that remarked on the social utility of ambition and the unintended public good that would spring under certain circumstances from ambition. Mercantilist and free-trade thinkers alike, then, agreed that it was unnecessary to sacrifice individual self-interest to the larger demands of "virtue." The issue that decisively divided them was the mercantilist contention that enlightened political authority could ensure that the widespread pursuit of self-interest resulted in the good of the nation as a whole. In the eighteenth century debate about the proper role of government in commercial societies, there can be little question that Mandeville stood with the mercantilists. Hence, his role in promoting the "spirit of capitalism" ultimately requires a more complex analysis than one will find in this book.

RICHARD F. TEICHGRAEBER III

Tulane University

Marxism and Totality: The Adventures of a Concept from Lukács to Habermas. By Martin Jay (Berkeley and Los Angeles: University of California Press, 1984. xi, 576p. $29.50 cloth; $14.95 paper).

Both politically and philosophically Marxism is not a very popular theme in Western societies today. There are many reasons for this situation, some having to do with the emergence of novel or post-Marxist perspectives, but the neglect carries a price. Martin Jay's study reminds us of the rich intellectual resources stored up in the Marxist tradition. Unlike Kolakowski, Jay does not presume to offer a survey of all the "main currents" of this tradition, nor does he approach his topic from a position of radical disaffection. Instead, his book offers a sympathetic, but non-sectarian treatment of one major strand in this legacy, namely, "Western Marxism"—a strand strongly indebted to the "philosophical critique that began with Kant and German Idealism" and which, on the whole, was resolutely opposed "not only to the fatalistic economism of the Second International, but also to the voluntarist vanguardism of the Third" (p. 2). In seeking to distill the central concerns of this strand, the book does not present a panoramic survey, but rather relies on one central category—the concept of totality—as a guiding "compass to help us traverse the vast and uncharted intellectual territory that is Western Marxism." Although acknowledging the array

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of other key concepts (like praxis, value, or dialectics), Jay argues that "the issue of totality has been at the center of the Marxist, or at least Western Marxist, debate as it has not been with bourgeois thought" and that "by spelling out the various meanings of totality and investigating their implications for other aspects of Marxist theory, we can fruitfully make sense of the tradition in new and revealing ways" (p. 14).

The study is impressive both in its meticulous scholarship and its philosophical subtlety and acumen. Together with his numerous other publications, *Marxism and Totality* establishes Jay as an intellectual historian and interpreter of the first rank. To be sure, intellectual historiography is always fraught with the tension between descriptive narrative and engaged philosophical argument. As Jay's work shows, however, the tension need not be debilitating: his new study succeeds remarkably well on both levels. In the case of his chosen topic—Marxism—the temptation to reduce philosophical ideas to their genesis or social-economic context has traditionally been very powerful, but Jay does not succumb to it. In terms of methodology, he shows himself an astute student of hermeneutics and post-hermeneutics from Gadamer and Ricoeur to Derrida—both with regard to the creative role of the interpreter and the "intertextual" relation of text and context. Without converting Marxist texts into abstract thought systems, he writes, "I nonetheless want to respect the arguments they contain rather than treat them as mere symptoms of external circumstances. Insofar as intellectual historians have come to recognize contexts as themselves texts requiring hermeneutic decipherment, we can no longer expect answers by referring to an unproblematic social reality fully external to the works we examine" (p. 17). Following Timothy Bahti, he calls his approach *Nachkonstruktion* rather than simple *Rekonstruktion*: "Instead of being a neutral recording of history 'as it actually happened' . . . it will be a rhetorical reenactment shaped by my own concerns and experiences."

The historical terrain traversed by the study is vast and complex. I can retrace here only the barest outline. Viewed from the perspective of the concept of totality, the story is basically one of rise and decline—followed by a more moderate resurgence in recent times. After a brief sketch of the "topography" of Western Marxism, the study opens with a survey of the "discourse of totality" in Western philosophy before and after Marx—a survey stretching from Spinoza, Vico and the Enlightenment over Rousseau and German Idealism to Marx and the diverse trends of "bourgeois" thought in the last century (including Comte, Dilthey, and Weber). A tour de force, the chapter by itself warrants acquisition of the book. The story of Western Marxism begins with the formulation of a strong version of "totality" by three thinkers variously influenced by Hegel: Lukács, Korsch, and Gramsci. After a detour through Bloch's utopian naturalism, Jay portrays the progressive disintegration of Hegelian or "expressive holism" in three main settings or contexts: the German, French, and Italian. In the German case, the journey leads from Horkheimer's slow retreat from revolutionary élan over Marcuse's "anamnestic" holism to Adorno's post-Hegelian concern with non-identity and negative dialectics. In the French context, the study moves from the Hegelianism of Lefebvre and Goldmann over Sartre's existentialist Marxism and the ambiguous holism of Merleau-Ponty to the radical anti-Hegelian posture of Althusser's structuralism. The Italian study proceeds from Gramsci to the quasi-positivist scientism of Della Volpe and Colletti. As a counterpoise to the disintegration of the Hegelian legacy the study finally examines the linguistic or communicative "reconstruction" of Marxist holism by Habermas. An epilogue discusses the challenge hurled at any type of philosophical totality or totalization by recent post-structuralism and deconstructive analysis.

Marxism and Totality is too rich to permit a detailed acknowledgement of its strengths or virtues. Gramsci students may find intriguing the distinction between two holism's in his work: an idealist, Crocean and a non-idealist, Vichian type. Equally stimulating and insightful are the comments on Surrealism (as a backdrop to Lefebvre) and on Marxist aesthetics (in the Goldmann chapter). Its merits overshadow the study's quandaries and unresolved issues, of which I can mention only a few. Given the pivotal role of the Frankfurt School in the story of Western Marxism, close attention must be paid to accents set in the presentation of its key figures. On the whole, the older generation of critical theory tends to be

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assessed from a Habermasian perspective, for which Jay repeatedly voices sympathy. Despite a perceptive treatment of non-identity, the study finds that "Adorno's work inevitably led into a kind of cul-de-sac." This failure is traced basically to the neglect of communication, the "hostility to the dialogic, communicative function of language" (pp. 272, 274): "The possibility of a non-hypostatized intersubjectivity, a public realm in which non-coercive, rational discourse might take place among equals, was absent from his thinking, as it was in Critical Theory as a whole until Habermas." The chapter devoted to Habermas accordingly pays tribute to the "linguistic turn" in the latter's work and his stress on communicative rationality. In light of this evaluative slant, the reader is liable to be perplexed by the conclusion of the same chapter which raises doubts about Habermas's view of language (p. 508): "What if language is conceived as having more than one telos, other than the goal of achieving undistorted speech? What if the metaphoric, polysemic, playful capacity of language is highlighted, rather than its discursive function? . . . What if language's deep structure, which Habermas has so far neglected to study, can be understood to work against or even undermine the pragmatic utterances that strive for perfect communicability?"

As Jay realizes, questions of this kind have been forcefully articulated by spokesmen of contemporary post-structuralism, including Derrida, Foucault, Lacan, and Deleuze. The Epilogue shows that the "challenge of post-structuralism" extends not only to the telos of "communicability" but to all forms of holism, Hegelian or otherwise, and frequently to Marxism as such. Moreover, the intellectual challenge is reinforced in our time by real-life experiences, including the disclosures about the Gulag. While suggestive, Jay's concluding remarks on these issues are somewhat pale. If the threat of totalitarianism as well as the "negative totality of nuclear catastrophe" are to be avoided, he writes, "we may well need to find some positive alternative" (p. 537). Curiously, a phrase from Adorno serves to highlight this alternative: "What can oppose the decline of the West is not a resurrected culture, but the utopia that is silently contained in the image of its decline." As it seems to me, unearthing this utopia requires today a post-Hegelian, but by no means anti-Hegelian reflection which resists positive totalization without lapsing into random particularism. Glimpses of this reflection can be found not only in post-structuralism but also in various post-metaphysical arguments from Levinas to Heidegger and Gadamer. In this light, much if not everything depends on the confrontation and reconciliation of modern rationalist holism and post-metaphysics—a point on which Jay seems to agree: "On that confrontation, I would contend in conclusion, rests the most likely possibility of a viable reconstitution of the Western Marxist tradition and, in particular, its much beleaguered concept of totality" (p. 509).

FRED DALLMAYR

University of Notre Dame

The Rhetoric of "Leviathan": Thomas Hobbes and the Politics of Cultural Transformation. By David Johnston (Princeton: Princeton University Press, 1986. xx, 2334p. $25.00).

Hobbes and the Social Contract Tradition. By Jean Hampton (Cambridge: Cambridge University Press, 1986. xii, 299p. $42.50).

David Johnston's marvelously clear and lucid study examines the political teachings of *Leviathan* against the background of Hobbes's early writings, especially the translation of Thucydides (1628) and the *Elements of Law* (1640). Unlike conventional treatments that posit their own versions of an "epistemological break" between Hobbes's early humanistic and later scientific works, Johnston proposes a more complicated genealogy. From Thucydides and the ancients, Hobbes acquired a keen sense of the weakness of reason and logical demonstration as a means of political reform. While the axiomatic methods of philosophy (or science) had greater explanatory power, the more visual imagery of history and political oratory had greater practical efficacy. Political history would seem, then, to be the privileged mode of discourse for writers with a practical intent.

Johnston argues that Hobbes did not so much change his mind as shift his allegiance in the *Elements*, a sort of draft copy of *Leviathan*. Here Hobbes argued that rhetoric is the most dangerous enemy of reason and clarity. By

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appealing to the passions, rhetoric deluded men into ignoring what had become for him the one axiomatic premise of human nature, namely that death is the greatest evil, and the policy of rational egoism that followed from this. But it was only in *Leviathan*, Hobbes's masterpiece of political philosophy, that he sought to synthesize the methods of the new science with the lessons of the older rhetorical and humanistic tradition. In *Leviathan* Hobbes developed a new kind of political rhetoric, one suited to turning the abstract deductive model of man sketched in the *Elements* into a practical program of political action. While Johnston remarks that "it would be wrong to suggest that *Leviathan* is *not* a work of science or philosophy," he goes on to suggest that Hobbes reversed his earlier priorities and allowed his political ambitions to dominate. "In writing this work," Johnston observes, "Hobbes was, above all else, performing a political act, not a scientific one" (pp. 67, 70, 91).

The largest portion of the book (chaps. 4–8) is devoted to an exposition of Hobbes's often neglected critique of theology in Parts 3 and 4 of *Leviathan*. Here is Johnston's most significant innovation. In the past, Hobbes's concern with religion has been treated as a residual legacy of certain seventeenth century theological controversies with no essential connection to the formal structure of his philosophy. Recently interpreters like Warrender and Hood have argued that Hobbes's politics are fully compatible with the Thomistic tradition of natural law. Johnston, however, sees Hobbes's treatment of religion more as a form of ideology critique necessary to rid the mind of its magical and superstitious elements and replace them with more "enlightened" views.

The dominance of the religious motif in Johnston's interpretation serves two functions. First, it allows us to see that reason and the politics of self-interest for which Hobbes is justly famous is not a "natural" propensity of man but an acquired skill. Man as he is is not a rational calculator but a being dominated by irrational beliefs in magic, prophecy, and religious superstitions. Thus men must be taught to be rational, to learn to fear death, if political society in the future is to be stable and peaceful. Second, because religious fears remain the single greatest obstacle to the realization of Hobbes's political project, Johnston is enabled to present him more as the embattled *philosophe*, the propagandist of Enlightenment, than as a detached scientific observer. What he calls Hobbes's "politics of cultural transformation" is aimed not just at demonstrating the limits of sovereignty and the duties of subjects but at rooting out various kinds of metaphysical fallacies and false beliefs that make the new order impossible.

Jean Hampton offers a radically different Hobbes. Her goal is not the historical elucidation of Hobbes's context and development but a "rational reconstruction" of his social contract theory informed throughout by the use of "contemporary philosophical and mathematical tools" especially game theory (p. 3). Unlike Johnston who sees Hobbes's text as in large part an exercise in political rhetoric, Hampton regards it as a work of systematic theory where arguments for absolute sovereignty follow deductively from premises regarding human nature, psychology, and moral theory.

According to Hampton, the core of Hobbes's political theory is his "alienation" model of the social contract where people simply give up or alienate their power to a political ruler in the hope that doing so will satisfy certain important human needs. Because the alienation must be total, it follows that subjects retain no rights against the sovereign should he fail to satisfy them. Hampton believes that Hobbes's alienation model fails because it requires us to alienate the very rationality that we need to bring the state into being. A more satisfactory model of the social contract is attributed to Locke who regarded the granting of power less as a surrender than as a loan. According to the "agency" model, which became central to later liberal notions about constitutional government, the relationship between ruler and subject is not that of master to slave but of agent to principal where the ruler or agent is still ultimately dependent upon the will of the principal (pp. 122–23, 256–84).

Obviously much depends on how one understands the term alienation. In both the *Elements of Law* and *De Cive* Hobbes treated the creation of sovereign power as requiring the complete renunciation of rights by the subjects. However, in *Leviathan*, Hobbes used the term "authorization" to describe this transfer of power that implies actively appointing someone to be one's agent, not just passively surrendering one's rights. To say, then, that

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the sovereign's power is authorized by the subjects is, at least potentially, to limit that power by the consent of the governed.

Now here is a problem. For Hampton to save her thesis she has to argue that authorization in *Leviathan* is compatible with, even implied by, the full surrender argument developed in Hobbes's early works. Against revisionist scholars like David Gautier, she maintains that in the seventeenth century even proponents of parliamentary government, like Phillip Hunton, understood authorization as implying the total alienation of one's rights to the sovereign. What Hobbes, Hunton, and later Kant were all trying, unsuccessfully, to come to terms with was the "paradox of being governed"—how people can be both the creators of the state and its subjects at the same time—which Hampton seeks to "dissolve" in her final chapter (pp. 279–84).

The question that Hampton finds baffling is why Hobbes remained wedded to the social contract methodology when its "agency implications" clearly violated the absolutist politics he tried to defend. Johnston's answer here is, I think, the more plausible. He suggests that for Hobbes sovereign authorization was a way of meeting the nascent democratic demand that the rights of the sovereign be recognized as legitimate by the people (p. 83). Hampton, by contrast, is forced to present Hobbes as a deeply incoherent thinker who would have done better adopting the divine politics of Robert Filmer (or Plato in the *Crito*) to defend absolutist government (pp. 264–65, 267).

It is tempting to play these books off against one another. Because I find Hobbes more of a political thinker than a game theorist, I find Johnston's analysis more satisfying. I agree with Hampton that the "tools" of game theory and modern analytical techniques may be useful in helping to elucidate what a thinker had to say but more often they simply get in the way, preselecting the questions deemed to be important as well as the answers the thinker should have supplied. Thus, at the very outset of her book we read: "Of course, in one sense, most of us in the twentieth century are already confident that [Hobbes's] argument fails in some way, for we believe that there is no successful argument for a polity as distasteful to us as absolute sovereignty" (p. 3). But if it is already predetermined that his argument "fails" just because "most of us" find it "distasteful" then this is more of an obstacle to understanding than an aid. The result is a work that reconfirms contemporary prejudices about Hobbes but does not get to the core of his teaching.

The fact is that whatever the logical structure of Hobbes's argument, we live in a world that has been decisively shaped by his perspective. Unlike the ancients, Hobbes along with Bacon, Descartes, and Spinoza regarded his philosophy as part of a "project" that should be judged by its "fruits," mainly the conquest of nature and the relief of humankind's estate. The purpose of this project was to educate society regarding the potential benefits of the new science that would be put toward securing the enlightened ends of "peaceable, sociable, and comfortable living" (*Leviathan*, chap. 15). That so many today can even think like game theorists, can pursue strategies of rational egoism, and assume such behavior to be "natural" is a stunning testimony to Hobbes's success. One can only wonder whether the consequences of his success have been altogether benign.

STEVEN B. SMITH

Yale University

Arguing for Socialism: Theoretical Considerations. By Andrew Levine (Boston: Routledge & Kegan Paul, 1984. xiv, 241p. $23.50).

Academic philosophers of the analytic persuasion have already transformed the modern liberal-democratic tradition from a lively, largely ideological, real world debate into a set of abstract postulates and propositions. Philosophy now seems bent on performing a similar alchemical transformation of socialist theory, judging from Andrew Levine's book. Levine has two distinct, although related, aims: first, to demonstrate the theoretical superiority of socialism over capitalism in terms of the "dominant tradition" of liberal-democratic values; and second, to affirm the "rational kernel" of a specifically Marxian version of historical change towards socialism. My objections to *Arguing for Socialism* do not stem from what may be regarded as Levine's ultimate conclusion: that political will is the essential ingredient in the translation of democratic socialist

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theory into practice—and that victory over capitalism and state socialism is by no means assured. But Levine's book unintentionally reveals that analytic philosophy—although not philosophy in general, I think—is of negligible value in accomplishing this change.

Levine's difficulties begin with his introduction, where he produces excessively economistic "stipulative definitions" of capitalism and socialism (pp. 5–10). His rationale for the definitions he offers is typical of the ordinary language analyst; they are meant to cover all existing cases of capitalism and socialism, as well as feasible alternative models. Thus, capitalism is necessarily and sufficiently determined by the absence of market-related political unfreedom and the presence of private ownership of the means of production. Analogously, socialism is understood solely in terms of deprivatized, public ownership of productive means. Apparently neither economic system implies any characteristic constituent political structure whatsoever. In Levine's view, the bare economic similarities which seem to be shared by nations that claim to be either capitalist or socialist are sufficient to distinguish capitalism from socialism.

Why we should be compelled to accept these definitions on the basis of self-ascription is unfathomable. Levine refuses to apply a critical and discerning eye to the actual conditions of contemporary societies. Ideological distortion is not an eventuality with which Levine's philosophical method prepares him to cope. Consequently, his conceptualization of socialism and capitalism is logically rigorous but historically naive. More significantly, Levine champions a deceptive separation of the economic and the political. Neither capitalism nor socialism is compatible with just *any* political structures. Rather, the capitalist state, regardless of how constituted, must perform certain functions of a juridical-coercive nature inseparable from the "economic" arrangements of capitalism. Similarly, the realization of socialism arguably requires a political apparatus for worker self-administration and self-control; any system which does not allow for these activities cannot be reconciled with public ownership.

Another central problem of *Arguing for Socialism* is the inconclusiveness of its comparisons of capitalism and socialism. In part, this is because Levine invokes as his theoretical yardstick "the dominant tradition" of *bourgeois* philosophy, which he treats as an independent and intractable system of values. But are capitalism and socialism even theoretically comparable? Socialism (or, at least, the democratic variety he desires) necessarily requires a future-directed theory in order to bridge the gulf to practice. But because capitalism is already well entrenched, its theoretical constructs perform the conservative function of abstract legitimation. Insofar as socialist and capitalist theories have such different orientations, any genuinely "objective" comparative evaluation must await the unknown outcome of historical development.

Arguing for Socialism is indeed an apt title. For Levine cannot see his way clear to do anything to imbue his arguments with practical significance. Formulating alternative strategies and groundwork for the transformation of society is the task demanded of socialist philosophy today. The ramifications of such historical change are far too important to be left to a Pascalian wager.

CARY J. NEDERMAN

University of Canterbury

Marx, Nietzsche, and Modernity. By Nancy S. Love (New York: Columbia University Press, 1986. xii, 264p. $27.50).

Nancy Love's comparison of the visions of Marx and Nietzsche is a self-proclaimed attempt (1) to illuminate "the problem of modernity," (2) to establish the limitations of theoretical syntheses of these two intellectual titans, and (3) to contribute to the debate over the relationship between Nietzsche's philosophical and political thought currently being carried on in the work of Robert Eden, Mark Warren, Richard Schact, Ofelia Schutte and others (pp. 8–11, 207). The second of these goals clearly dominates her study. Love's effort to achieve this goal, however, is characterized by a propensity for abstraction. She has, for instance, laced her study with an abundance of fairly long and intriguing quotes that indicate a more than passing familiarity with the writings of Marx and Nietzsche. Nonetheless, it is precisely this overreliance on quotations, many of which are left to stand by themselves, that is the most obvious sign of Love's afflic-

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tion with a major "problem" of modernity to which we are all susceptible—what Weber called intellectualization.

Evidence of this problem may be seen in Love's use of Weber's notion of ideal-type, which is, oddly, not explicitly introduced until the closing pages of her study, to explain why she avoids addressing specific syntheses of Marx and Nietzsche. "I have not criticized any specific attempt to combine Marx and Nietzsche," Love concludes, "but rather have tried to demonstrate the contradictions between their perspectives" (p. 201). What are these contradictions? According to Love, there are three fundamental differences that her "ideal-type synthesizer" (or "abstraction") ignores between Marxian science and Nietzschean skepticism (chap. 4), Marx's socialist and Nietzsche's individualistic critiques of liberal democracy (chap. 5), and opposing visions of liberation through production and play (chap. 6). And why does this "synthesizer" overlook such contradictions? According to Love, Weber attempts synthesis because both Marx and Nietzsche criticized German idealism and analyzed how humanity creates itself historically (chap. 2). Both also had similar theories of societal development that rejected strictly teleological and causal explanations in favor of functional analyses of the growth of human powers over nature and society (chap. 3). Nonetheless, attempts to combine Marx and Nietzsche, Love reminds us more than once, result in "schizophrenia, not synthesis" (pp. 17, 200).

Love's refusal to break free of the logic of her schizoid synthesizer prevents her from addressing this very important topic in a more sensible manner and strengthening her case against theoretical synthesis. Had she probed more deeply a few of her numerous citations from Marx and Nietzsche, she might have discovered that her three contradictions are not as neat as they at first appear. Barring her own unpacking, Love might have raised significant questions about the oppositions of science and skepticism by directly addressing the work of Hans Barth (p. 12); socialism and individualism through the writing of Louis Dumont; and production and play by reviewing the thought of Herbert Marcuse (p. 235). But the abstract reason of the schizoid synthesizer subsumes all particular theoretical critics.

Love's failure to counter criticisms of her analysis leaves her defenseless before the similarities encouraging synthesis that she abumbrates in chapters 2–3; for Marx's and Nietzsche's revolutionary theories of "man's self-creation" (p. 31) have armed us with weapons for transforming the world, society, and the self that have striking affinities. According to both visions, a good theory confers power because it creates options. The radical opening of possibilities encouraged by such theories may well be the key to what Love calls "the problem of modernity."

ALAN WOOLFOLK

University of Southern Mississippi

Civil Religion and Political Theology. Edited by Leroy S. Rouner (South Bend, IN: University of Notre Dame Press, 1986. xv, 228p. $24.95).

In *The City of God* Augustine struggles with a question which perplexed even the earliest Christians: What does the practice of religion have to do with the powers of the earth? *Civil Religion and Political Theology* treats this problem by focusing on the fragile relationship of political theology and public philosophy, and by using the United States as a case study for clarifying the pitfalls and possibilities of this relationship.

The names of many of the contributors to this volume (vol. 7 in the Boston University Studies in Philosophy and Religion series) will be familiar even to those whose knowledge of the subject is cursory—Jurgen Moltmann, Robert Bellah, Richard Neuhaus, John Wilson, Johann Metz, James Cone, John Cobb. Because the essays were originally lectures critiqued by a panel of scholars and revised for publication, they display an inner coherence uncharacteristic of collections of this sort.

The assumption shared by all the essays is that "religion must be relevant to political life without being co-opted by it," and the theme they all address is "the role Christianity does and should play in the life of the polis" (p. 2). This theme is interpreted from three angles that dictate the three major divisions of the book: the common intellectual foundations which make possible the practice of democratic values; the way civil religion based on these common values manifests itself, for bet-

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ter or worse, in the American experiment; and several ways in which the values of political theology are practiced as modes of political liberation and economic justice.

Although these essays are excellent expositions, and indeed interesting for those not familiar with the terrain, they are for the most part all too familiar. There comes a time when repeating a conducted tour tends to weary the intellect rather than excite it. This is not to say that the authors are dull or incompetent. They are wholly competent as expositors. It is rather that these essays do not advance the discussion very far, especially those essays which focus on the nature and role of civil religion in America. One of several exceptions is John Cobb's contribution "Christianity, Political Theology, and the Economic Future." Cobb is bipartisan; on the basis of Christian beliefs and values, he assesses the strengths and weaknesses of capitalism and socialism, "calls for new theories" and suggests "some of the considerations important for their formation" (p. 223).

Another matter detracts from the value of this volume. Political theology as liberation theology, the theme of the last section of the volume, not only excludes the Latin American perspective which has had an enormous impact on the formation of American liberation theology (e.g. the Roman Catholic Bishops), but also excludes Jewish liberation theology which in a fundamental theological and historical way (namely, the Exodus) is the paradigm for most political theology in the Western tradition. Except for James Cone who recognizes that black liberation is partly rooted in "exodus-liberation," this volume ignores (as is typical of volumes of this kind) the contribution of Jewish thought to the formation and practice of Christian political-liberation theology.

JAMES E. GILMAN

Mary Baldwin College

AMERICAN POLITICS

The False Promise of Administrative Reform: Implementing Quality Control in Welfare. By Evelyn Brodkin (Philadelphia: Temple University Press, 1986. x, 156p. $29.95).

In the past two decades, welfare policy and administration has been at the center of much public controversy. Early demands that the "rights" of welfare claimants be respected were soon drowned out by criticisms of the "welfare mess"—claims of rampant fraud and abuse alongside rapidly escalating costs and administrative inefficiency. Despite widespread concern, Congress has been unable to enact welfare reforms, because it cannot resolve conflicts between social responsibility to the disadvantaged and concerns to limit fraud and to ensure work incentives for claimants. As Evelyn Brodkin explains in her excellent book, however, congressional failure did not prevent the reform of welfare administration. Both the Nixon and Carter administrations adopted quality-control programs that, while ostensibly aimed at reducing error rates in welfare decisions, also caused substantial changes in the operation of welfare programs.

Brodkin neatly combines very different theoretical perspectives in explaining welfare politics. She begins with the "governability crisis theory" of Samuel Huntington and others: neither the Congress nor the executive can resolve political conflicts over welfare, so coherent and responsive national policies cannot be formulated. She combines this pluralist analysis with the neo-Marxist analysis of Claus Offe that when political deadlocks occur, alternative forms of political resolution develop, such as administrative reform. Brodkin describes the debate over welfare policy in the 1970s, showing how it was converted, particularly by the Nixon administration, into the "technical question" of quality control after direct efforts at reforms were frustrated. Welfare policy politics were made "more manageable by: (1) de-politicizing policy choices, (2) limiting participation in the policy-making

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process, (3) making policy without appearing to, and (4) legitimating administrative practices on the basis of widely held, nonpolitical values" (p. 22).

Brodkin then uses the ideas of street-level bureaucracy to explain how quality control affected the delivery of welfare. Concerns about welfare fraud and abuse motivated quality-control efforts, focusing it on procedural compliance and errors of liberality (overpayments and payments to ineligible claimants). This focus, backed by threats of financial sanctions by HEW, made the states concentrate on procedures and errors of liberality without recognizing the resulting increase in errors of stringency and new barriers that discouraged some eligible clients from applying. In a case study of Massachusetts, she explains how the threat of sanctions led the state to give district welfare offices strong incentives to reduce quality-control errors while maintaining or increasing productivity. Welfare offices in turn tied caseworkers' evaluations and promotions to measures of productivity and quality-control errors. The street-level bureaucracy perspective generally points to restrictions on managers' abilities to control case-level decisions, but Brodkin shows how the narrow focus of the quality-control effort altered decision making despite officials' considerable discretion. In another version of that familiar organizational story, the means—quality control—became the operative ends of administration.

I have only two quibbles with this book. First, Brodkin may have been too successful in presenting her arguments and data concisely. She writes lucidly and her analysis is rich, so that in some places I wish she had gone into more depth. Second, she is not wholly persuasive in her argument that officials adopted quality control *in order to* restrict access to welfare. She shows that the effect of quality control was to restrict access, but it is not sufficient to assume that policymakers intended those effects, particularly as she explains how the Carter administration adopted quality control to remove the taint of fraud and inefficiency that might provoke reductions in congressional and state support for welfare.

Brodkin has written a compelling study of the administrative policy process with important conclusions that extend beyond the confines of welfare policy. Studies of social services and government administration generally would do well to incorporate her concern with the relations between policy-making and administration and her careful working-through of the politics and consequences of administrative reform.

JACK TWEEDIE

State University of New York, Binghamton

Presidential Communication: Description and Analysis. By Robert E. Denton, Jr. and Dan F. Hahn (New York: Praeger, 1986. xxiv, 332p. $39.95 cloth; $15.95 paper).

"The purpose of our study," Robert Denton and Dan Hahn note in the preface of this work, "is to investigate the institutional presidency from the traditional perspectives of communication scholarship" (p. xiv). One cannot but be impressed by the authors' thoroughness as they go about developing their thesis that "communication—its quantity and quality—defines the nature and essential characteristics of the U.S. presidency" (p. xii). In fact, one of the major strengths of this study is that Denton and Hahn have fashioned their work on a firm and solid foundation of scholarly research in the areas of communication theory, history, and political science. Without doubt, this work is one of the most thoroughly researched yet readable books on the presidency to be published in the last 25 years.

Denton and Hahn observe and investigate the presidency from interpersonal, intrapersonal, small group, and mass communication orientations. One chapter in the book is devoted to each of these perspectives. The overall quality of the study is enhanced in that the authors have included various readings or ministudies that illustrate and elaborate upon the points made and conclusions drawn in each chapter. These readings and ministudies, appropriately placed at the end of every chapter, are very instructive in that they link theory and political reality.

The chapters dealing with the intrapersonal and small group communication dimensions of the presidency are especially interesting and informative in that they address aspects that warrant more attention and study by today's scholars. Denton and Hahn do a superb job of dealing with these areas of presidential com-

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munication and raise several intriguing points and questions for their readers to ponder. For example, they argue that "ironically, a strong president may be the least effective user of (small) group communication" (p. 239).

This work is unique in that "it is the *only* text that focuses on communication variables in relation to the presidency" (p. xv). There is no question that Denton and Hahn have made a significant contribution. Their work may soon join Richard Neustadt's classic *Presidential Power: The Politics of Leadership* as one of the most influential publications in the area of presidential studies.

C. DON LIVINGSTON

Western Carolina University

Exploring Federalism. By Daniel J. Elazar (Tuscaloosa: University of Alabama Press, 1987. xvi, 335p. $28.95).

No contemporary scholar has observed as many federations at first hand or written so prolifically about federalism as has Daniel Elazar. Apart from his writings, his intellectual leadership is embodied in the work of the Center for the Study of Federalism, which is celebrating its twentieth anniversary at Temple University under his directorship and in his efforts as the founding editor of *Publius*, the quarterly journal of federalism. *Exploring Federalism* is the fruit of Elazar's mature experience and reflection and is a dauntingly comprehensive and erudite book.

Elazar makes bold claims for federalism in both its philosophical-theological and institutional dimensions. According to him, the roots of federalism are in the partnership between God and the Hebrew people as elaborated in the Pentateuch and the subsequent agreement among individuals and families to found polities based on consent. In its institutional manifestation, the characteristic federal relation is based on a "matrix," a fragmentation of power, rather than hierarchical power relations. Federalism not only embodies durable values of liberty and justice but accords closely with the findings of contemporary cybernetics, developmental psychology, and anthropology about human nature and human society. Thus federalism "embodies a world-view and an approach to the study of political phenomena that come close to being comprehensive" (p. 29).

I do not find persuasive Elazar's attempt to rewrite the standard accounts of the history of political theory to elevate federalism to a first-order concept akin to natural law and natural right (p. 1). Apart from some attention to Althusius, he has little to say about major figures in the history of political thought. Proudhon, who is often regarded as a major federalist thinker, is relegated to a short paragraph. Significantly, Elazar finds "fragments" of federalism in Rousseau's *Social Contract*, although one might persuasively argue that of all the concepts of political thought the *general will* is the one most directly in contradiction to federalism. One contemporary thinker who *does* advance federalism as a first-order political idea is Pierre Trudeau in his association between federalism and reason and his sensitive analysis of the relation between federalism and rationalism (*Federalism and the French Canadians* [Toronto: Macmillan, 1968]). In general, G. F. Sawer's analysis of the place of federalism in the conflict among political ideas is more accurate than Elazar's account: "People are not likely to go to the stake or the barricades to defend federalism as such. They may undertake heroic actions for the sake of some value which federalism happens at the moment to favour and may then even inscribe federalism on their banner—Liberty and federalism—Equality and federalism—but never just Federalism" (*Modern Federalism* [London: C. A. Watts, 1969], 186–87).

In his enthusiasm for federalism and his emphasis on its consensual aspects, Elazar refuses to face the extent to which federations have been maintained by physical violence and federal forms used to buttress illiberal institutions. After all, of the three most populous contemporary federations, the United States and Nigeria have been sustained by the bloody defeat of secession, and India has relied on the role of physical force in maintaining that nation's continuing existence. Federal jurisdictional guarantees have supported illiberal governments in, for example, Huey Long's Louisiana, Duplessis's Quebec and Bjelke-Petersen's Queensland. William Riker ended his 1964 book on federalism in eight nations with this statement: "If in the United States one disapproves of racism, one should disapprove of federalism" (*Federalism: Origin Operation,*

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Significance [Boston: Little, Brown, 1964]. Despite the allegedly invariable relation between federalism and pluralism, which pervades Elazar's analysis, at the time this review was being written, several of the provinces were in process of denying the Mulroney administration the consent it sought to amend the Canadian constitution to recognize the rights of aboriginals to self-government.

On the whole, however, Elazar is justified in his judgment that federalism "minimizes the use of coercion." Interestingly, he sees the American cowboy "as the most prominent manifestation of federal political culture" because this person addresses other cowboys as *pardner* and "is able to work for others as a hired hand without surrendering an iota of his independence" (p. 193). Perhaps, but some of us associate the cowboy with a kind of lawlessness unusual to federalism, and A. V. Dicey's judgment may be sounder than Elazar's, that a federal system can flourish only among communities imbued with a legal spirit and trained to revere the law. Federalism substitutes litigation for legislation, and none but a law-fearing people will be inclined to regard the decision of a court as equivalent to an enactment a "law" (*Introduction to the Study of Law of the Constitution*, 7th ed. [London: Macmillan, 1908], p. 140).

Elazar's knowledge of contemporary political systems is formidable. However, the scope of *federalism*, as he comprehensively defines it, is such that no individual scholar can write authoritatively on all its aspects. Thus it is just not true that Australian state constitutional law "is of little significance" (p. 177) and the assertion that Rufus Davis approaches federalism in "the classic manner of English scholarship" (p. 28) flies in the face of the circumstance that Davis's writings are based on a first-hand knowledge of the Australian federal experience. There is little realization, in the account of federal party systems, that Canada, unlike such other parliamentary federations as India and Australia, has almost completely divorced the major party organizations geared to federal and provincial electoral competition. In respect to the United States, while the past influence of the Supreme Court on the federal system has been decisive (p. 183), the Court has almost ceased to delineate the respective boundaries of federal and state powers, and, unless the *Garcia* decision of 1985 is reversed, this will become a permanent feature of U.S. federalism.

Exploring Federalism contains many really suggestive hypotheses that are not followed up with the detail they deserve. The statement, "The constituent polities in a federal system must be fairly equal in population and wealth or at least balanced geographically or numerically in their inequalities if non-centralizatiaon is to be maintained" (p. 170) raises an intriguing and practical question of great complexity. "True federal systems do not have capitals, they have seats of government" (p. 75), and Elazar deplores that Washington is increasingly being referred to as the nation's capital although it might reasonably be claimed that the center of U.S. imperial power should be so regarded; throughout the book there is little sensitivity to the nexus between superpower status and U.S. federalism. At the end of the book there is a cryptic suggestion that U.S. citizens have jettisoned the "constitutional dimension" of federalism and thus the capacity to control and manage bureaucracies. This tantalizing suggestion invites further analysis.

Elazar makes a richly documented and persuasive case that liberty and justice are best served by a fragmentation of political power. More controversially, perhaps, he takes the position that effective public policies are encouraged by such fragmentation against centralized and bureaucratized structures. Elazar casts the net of federalism more widely than would most scholars and he is somewhat uncritical of federalism's deficiencies, but his book is an important contribution to the literature on the subject.

DONALD SMILEY

York University, Toronto

The Politics of Realignment: Party Change in the Mountain West. Edited by Peter F. Galderisi, Michael S. Lyons, Randy T. Simmons, and John G. Francis (Boulder: Westview, 1987. xi, 235p. $23.50).

The eight states of the Mountain West—Arizona, Colorado, Idaho, Montana, Nevada, New Mexico, Utah, and Wyoming—contain nearly one-quarter of the land area in the United States but only 5% of the nation's population. It is not surprising, then, that the

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political happenings of the region have not been a major political science agenda item. Still, the politics of the area can be of interest to more than those political scientists interested in the collection of esoteric data for no other reason than that the federal system overrepresents the Mountain West in the Senate by over 300%. One of every six senators is elected from the Mountain West.

The Politics of Realignment is clearly the best political science study of the area of which I am aware. The book, a collection of 10 essays, has as its focus the rise to dominance of the Republican party. For much of the twentieth century the Democratic party was highly successful in the Mountain West. As several of the book's authors point out, only the South was more favorable to the Democrats. Today, judging by a variety of measures, the Mountain West is by far the most Republican region in the nation.

The objective of this book is to describe the course and nature of the party realignment and to speculate about its causes. In addition, the main authors have done an excellent job of fitting the Mountain West case into the vast literature on party realignment. Basically, they conclude that the current realignment in the Mountain West is quite unlike previous party realignments. As they point out, the realignment has not been marked by a "political earthquake" such as the Great Depression. It has been a slow, fragmented movement in which the Republicans targeted Democratic Senate seats and concentrated on open House seats. The changing demographic composition and increased prosperity of the region; the increased policy polarization of the national parties; President Carter's attempt to reduce federal subsidies for western water projects; and the increased importance of television campaigning and changes in campaign finance made the drive to Republican ascendancy possible.

Although the various authors follow the book's theme very well, students of the region will find several of the chapters more useful and interesting than others. John Francis does an excellent job of providing a political overview. The chapters by Walter Stone, Arthur Miller, and Robert Benedict contain astute analysis and valuable data concerning changes in party identification and policy preferences. Gary Jacobson's examination of congressional elections and Henry Kenski's discussion of political campaigns are both valuable pieces of political analysis.

Political Realignment is remarkably free of factual errors considering that many of the authors do not reside in, and have not done much previous work on, the Mountain West. One exception can be found in the chapter by James L. Gibson. Using data from an article by Wright, Erikson, and McIver (*Journal of Politics* 47 [1985]: 469–89), Gibson states that the Democratic party is the stronger party in Wyoming. This piece of information is radically at odds with repeated surveys of the Wyoming population, party registration and election results since statehood. Surely the editors should have informed Gibson of the obvious error.

One inherent shortcoming of a work of this sort is that state variations are deemphasized. For instance, the Mormon influence in Utah, Nevada, and Idaho is a major factor in the explanation of politics in these states. One cannot understand New Mexico politics without considerable reference to its Hispanic heritage, a heritage that is absent from a state such as Montana. It might even be argued that to aggregate the eight states is to obscure much significant political data.

In all, however, *Political Realignment* is a most useful and valuable study of western politics.

OLIVER WALTER

University of Wyoming

Strangers or Friends: Principles for a New Alien Admission Policy. By Mark Gibney (Westport, CT: Greenwood, 1986. xv, 169p. $29.95).

This book attempts to develop the basis for a moral alien admissions policy. In part 1, Gibney briefly outlines the approaches of a number of political philosophers on issues ranging from the importance of community to human rights as they relate to the ethics of alien admissions. He then builds on these philosophical discussions to advance his own recommendations.

Gibney's argument is comprised of three fundamental ingredients. First, it takes as central the "Harm Principle" ("HP"); that is, that

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no individual (or, by extension, nation-state) should harm another individual and that if an individual (or nation) does harm another, it has a "special duty" to provide restitution to the victim. In addition, there is a less specific duty, according to Gibney's "Basic Rights Principle" ("BRP"), which mandates that even in the absence of culpability, we have a moral obligation "to honor the basic rights of subsistence and security that individuals possess" (p. 103). The third building block of Gibney's argument concerns the right of a community (generally synonymous here with *nation*) not to sacrifice "communal processes" in satisfying HP and BRP.

Taking these principles together and applying them to alien admissions, Gibney argues that the United States (or any country) should construct immigration–refugee policies that first favor those whom it has harmed via wars, foreign policy, and so on; that after this Harm Principle has been satisfied, it should offer admission to those whose basic rights are being violated, even if it has played no role in this violation; and, finally, that care should be taken in implementing HP and BRP (especially the less specific BRP), so that the receiving community is not disrupted. In order to minimize such disruption, Gibney suggests (1) that alien admission should only be considered after other types of aid have proven unsatisfactory; (2) that admissions should often be only temporary; and (3) that the Harm Principle and the Basic Rights Principle must not be interpreted too liberally, applying in most cases only when the victim is in danger of losing "life, limb, and vitality" (p. 142).

Perhaps the most consistent weakness of the book is the indiscriminate mixing together of philosophically based moral imperatives with descriptions of existing reality. For example, the primary defense of his recommendation that each developed nation give 1% of its GNP to alleviate suffering in other countries is that "many nations have already agreed to the 1 percent figure as a goal. . . . Perhaps 1 percent is not the 'right' figure, but it does not exceed what many nations think is the right answer." In light of Gibney's goal of building an ethics of aid based on moral principles, this defense of his ethics as no more than has already been done seems strikingly out of place.

Similar to such confusion between what "ought to be" and what "is" is a disconcerting mix of naïveté and hard-nosed pragmatism throughout the book. This problem can best be explored through an analysis of Gibney's statement "this system is not meant as an 'ideal' theory" (p. 147). On the one hand, Gibney's approach is indeed far from "ideal" in that, having set the normative basis for a moral admissions policy, he retreats into communal self-interest and an almost callous practicality in setting the limits on these admissions. For example, in his discussion of Mexican migration (p. 113), Gibney argues that while many aspiring Mexican migrants may live in poverty, they do not live "on the edge of subsistence" (a questionable evaluation). Therefore, they should be restricted, particularly, Gibney argues, since the United States has done no harm to Mexico (again, this is questionable), and therefore only the less stringent principle of BRP applies.

On the other hand, Gibney's approach is decidedly "ideal" in glossing over the difficulties of getting governments to act with a moral conscience. Indeed, it is here that the intermingling of the ideal with the real is the most perplexing. Speaking of the need for governments to accept as refugees those whom it has harmed with its foreign policies, Gibney takes the immorality (or at least self-interest) of given countries' foreign policies as relatively immutable, while assuming that in their refugee policies they can be convinced to become moral actors.

Gibney's treatment is also "ideal" in that it includes no analysis of the role of the state in Western capitalist societies, accepting instead the myth that public interest dominates policy-making. In spite of some reference to U.S. society as "pluralistic" (p. 15) and "diverse" (p. 16), he usually implies that policy-making is a consequence of "national goals" (p. 146), or what he refers to as the *trustee* arrangement, in which governments pursue the self-interests of their own citizens. In fact, he takes national self-interest to be the unfortunate reality of state decision making, the counterpart to his moral duties and obligations approach. Not only does this consensus view of decision making lead to a misinterpretation of past immigration policies as it underplays the role of economics and the conflict of interests based on economic factors, but this "idealism" vis-à-vis the U.S. state is probably also responsible for Gibney's failure to address the very real diffi-

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culties of implementing any of his recommendations.

KITTY CALAVITA

University of California, San Diego

American Public Opinion toward Israel and the Arab-Israeli Conflict. By Eytan Gilboa (Lexington, MA: D. C. Heath, 1987. 366p. $34.00).

Gilboa attempts a comprehensive review of U.S. public attitudes, as reflected in the polls, concerning Israel and Israeli-related issues. His survey begins around the mid-1940s and ends in 1985. The discussion is first presented in a chronological manner; in a second part, the author deals with "issues and group attitudes."

The main value of the book is that it represents the largest body of data on the subject, as well as attempting to analyze the polls on "Israel and the Arab-Israeli conflict" on the basis of the respondents' socioeconomic, ethnic, and religious backgrounds. Also, a brief section deals with "opinion trends."

In the introduction the author states that "this book questions not only whether public opinion affects American policy on the Middle East but also whether it affects the policies of Israel and other Middle Eastern countries" (p. 3). Unfortunately, however, there is no systematic treatment of these issues other than quotes, assertions, or speculations. Gilboa himself appears to be uncertain as to whether opinion follows policy (pp. 199, 317) or whether, as he more frequently asserts, public opinion affects, if it does not determine, U.S. policy on the Middle East (pp. 60, 95, 96, 155, 198, 229, 308, 320, 321) *as well as* the foreign policies of Israel and the Arab countries, especially Egypt (pp. 59, 63, 67, 74, 115, 161, 164, 320). In this regard, the author fails to cite perhaps the only study that deals with this issue systematically and concludes that U.S. images of Israel and Egypt tend to *follow* U.S. official policy (S. Iyengar and M. Suleiman, "Trends in Public Support for Egypt and Israel, 1956–1978," *American Politics Quarterly* 8 [1980]: 34–60).

While Gilboa amasses a great deal of information and data on the subject of attitudes, the study is definitely marred by certain flaws. First, the author—a professor at Hebrew University and the National Defense College of the Israeli army, as well as a consultant to the Israeli ministries of Foreign Affairs and Defense—uses the lengthy sections devoted to "appropriate historical contexts and perspectives" (p. 2) in order to defend, justify, or otherwise sympathetically explain Israeli positions or actions. Furthermore, the terminology is precisely that used by the Israeli government and its supporters. Thus, the 1973 war is always mentioned as the *Yom Kippur War*, the PLO and PLO activities are repeatedly designated as *terrorists* and *terrorism*, the Israeli annexation of East Jerusalem is refrred to as *unification* and its potential return to Arab control as *redivision*, and so on. Also, the 1967 Israeli attack on the *USS Liberty* is simply described as an accidental *mistake* (p. 45)—without any reference to the extensive published reports and allegations that it was deliberate. Finally, a more objective scholar might have difficulty making the unequivocal assertion, for instance, that "the War of Independence" and the 1967 Six-Day War were both "initiated by the Arabs" (p. 309).

A second flaw is Gilboa's treatment of the Palestinians merely as an "issue," similar to the treatment of "aid to Israel"; that is, they are not viewed as a people nor as a major party to the conflict. In fact almost invariably throughout the whole book, whenever the word *Palestinian* is mentioned it is as a "problem," an "issue," a "question," or in reference to "refugees" or "terrorists." While the Palestinians are not specifically and collectively called *terrorists*, the constant reference to "Palestinian terrorists" definitely contributes to that impression.

A third weakness is that while Gilboa's study appears to be comprehensive and while it includes a large number of polls commissioned by the American Jewish Committee, there is no mention of the 1982 poll commissioned by the Institute of Arab Studies and carried out by Decision/ Making/ Information, which provided questions with Arab, especially Palestinian, perspectives and concerns (*Arab Studies Quarterly* 4 [1982]: 358–74).

In the end, the author's concern to present Israel and Israeli activities in the best light possible gets in the way of an objective interpretation of the data. That there is strong support for Israel and sympathy for the Israelis among the U.S. public is clear and not in dis-

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pute. However, Gilboa goes beyond this and appears to be anxious to see to it that U.S. opinion of Israel be maintained at a high level, and he attempts to show that it is even stronger than it actually is. This is a major shortcoming and greatly detracts from the value of the book.

MICHAEL W. SULEIMAN

Kansas State University

Policy Design and the Politics of Implementation: The Case of Health Care in the American States. By Malcolm L. Goggin (Knoxville: University of Tennessee Press, 1987. xv, 296p. $34.50).

Malcolm Goggin has contributed to the transformation of *policy implementation* from a dirty word into a maturing—if not yet mature—subfield of political science. His book fits comfortably among a number of "second generation" studies that utilize comparative and longitudinal methods. It provides a more varied account of implementation than earlier single-case accounts that often chronicled policy debacles.

Goggin contrasts the "fizzle" of California's experience in the federal-state Early and Periodic Screening, Diagnosis, and Treatment (EPSDT) program with the "sparkle" of the state's Child Health and Disability Prevention (CHDP) program. He also examines EPSDT implementation in five other Sunbelt states. Finding enormous variation in the political and administrative processes of each case, he considers these findings in the context of four alternative models that explain why some states are more effective than others in implementing child health care programs. These models are not new but synthesize a diverse set of literature into a useful framework for analysis. Goggin ultimatelly concludes that organizational and personnel variables are more important to implementation success than are economic and political ones. He emphasizes, for example, that provider contentment and administrative stability may greatly exceed state economic conditions and policy liberalism in determining program success.

In *Policy Design and the Politics of Implementation,* Goggin also considers two additional issues that often go ignored in implementation studies. He attempts to address the economic and political realities within which programs must operate by considering major theories of power and its distribution in the United States. In addition, the book combines a most welcome assessment of the current limits on implementation research with a brief exploration of what "third generation" studies in this area might undertake. Goggin outlines a number of research alternatives. He stresses the need to develop long-term approaches and to "combine large and small-N studies" as has been common in other subfields.

Aside from its more general use in policy implementation, this book will be of particular interest to analysts of health politics. It is a fine supplement to other major works in health policy implementation. Goggin provides less breadth in coverage of health care programs and issues than books such as Frank Thompson's *Health Policy and the Bureaucracy* but offers a more detailed framework for further exploration. And the book is quite timely for current health policy debates, given its extensive analysis of major prevention-oriented health care programs focused on children. It provides sufficient evidence of program success to hearten advocates of new initiatives in these areas. At the same time, it contributes to the growing body of evidence that government efforts to promote prevention are no panacea in terms of health outcomes or health care cost containment.

Goggin also provides fodder for a debate that has become central to health policy. Federal and state efforts to expand health care services have historically relied on substantial deference to provider-group preferences to assure smooth implementation. However, that approach gave rise to extraordinary costs and resulted in more recent policies that give government greater control over providers. Goggin argues that a fundamental component of successful implementation in his cases is mainstream providers who are content with the program and consider themselves properly rewarded. In fact, he considers the more than doubling of physician fees in the California CHDP case to be perhaps the single most important factor in turning the program around. He suggests that expanded eligibility and open-ended financing commitments without spend-

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ing caps would best promote successful implementation of child health care programs. As such governmental expansiveness seems ever less likely in the late twentieth century, health policy implementation may prove even more troublesome.

Policy Design and the Politics of Implementation does not provide any definitive framework for examining changing contexts of implementation in health care or other spheres of domestic policy. Nor does it crack the cosmic egg of policy implementation that has attracted—and largely confounded—teams of scholars in the last 15 years. But it does constitute an important contribution to the incremental expansion of our understanding of the implementation process. Much more than a general list of guidelines for putting policies into operation, Goggin enriches the context in which we examine implementation, offers approaches that warrant emulation and expansion, and begins to chart the course of where we must head next.

BARRY G. RABE

University of Michigan

The Decline of Organized Labor in the United States. By Michael Goldfield (Chicago: University of Chicago Press. xv, 293p. $25.95).

This book is well written, understandable and thought-provoking. The author's opinion, which I find congenial, is that unions are good institutions and that we should be concerned about their decline. The approach is rather argumentative, in some places verging on polemic, but on the whole it is crisp and interesting. A large amount of factual information is provided in support of the author's claims.

The first contention, which is persuasively demonstrated, is that union membership is declining. The next chapters, which describe union weakness in politics and collective bargaining, are a distraction from the major topic of the book (which is the decline in membership). Some important questions are not carefully addressed. What conditions determine whether labor will win or lose in politics or bargaining? Why did labor succeed in politics when it was weak (during the New Deal) and lose when it was numerically stronger (1947 and 1959)?

The fourth chapter describes labor-management relations in the Marxist vocabulary of the class struggle and gives some reasons for concern about the decline of unions. In this discussion, the author's ideological predispositions are most evident. He notes that "union organization seems to be coincident with higher productivity, lower turnover, more efficient corporate practices, as well as many other social gains" (p. 76; based on the research of Freeman and Medoff) and that "the decline of trade unions casts a pall on the quality of life for the country as a whole" (p. 77). The contention is not widely accepted; without a more complete economic argument, it should not be. Others have argued that the post-1935 record of unionized firms is one of eroded markets share and that loss of profitability upon unionization is generally anticipated by stockholders. The discussion of labor-management conflict would be substantially richer if this conflict over the economic effect of unionization were elucidated.

The search for the causes of the decline in union membership takes up the second half of the book, which is the most important and serious analysis. Having shown that the prevailing theories (based on economic or sociological correlates of union membership) cannot explain the decline in membership, Goldfield concludes with his "class struggle" explanation of union decline. The argument is that labor law in the United States is unfavorable to unions, allowing employer participation (and interference) in the organizing process. A growing number of employer organizations fight unions with sophisticated legal (and illegal) techniques. The evidence of increased employer resistance during the past 30 years is quite substantial, though there is no reason it should be thought of as a class effort rather than an aggregation of individual business actions taken to prevent loss of profitability. The weakness in the explanation of membership decline is that though employer resistance is correlated with a reduction in union success, no persuasive explanation of the effect of employer activity on workers is offered.

Goldfield has not completely avoided the tendency of union membership studies to search for correlates rather than explanations.

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Before the circumstantial evidence linking employer opposition to changes in voting patterns can be accepted, basic theoretical questions should be addressed. How does a worker decide whether or not to vote for a union? How strongly are workers persuaded to vote for the union by campaign publicity? Why are some workers willing to risk their jobs by organizing a union, while others are afraid even to vote for a union in a secret ballot? The strong empirical relationships that Goldfield finds, for example, linking employer hostility to union losses (p. 203), would be much more persuasive if an explanation of the change in voting behavior were supplied.

At some points the explanation of the impact of employer resistance is not logically consistent. For example, it is noted that employers cause delays between the filing date and the representation election and that these delays are associated with union losses. Why do delays reduce union success rates? The only detailed study of representation elections, the Getman study, found that even unlawful employer campaign practices do not reduce union success rates. In fact, the campaign itself is usually rather unimportant. Goldfield's counterargument is that the Getman study overlooked the fact that the employer activities take their toll *before* the election filing has taken place (p. 199). If this is correct (i.e., if the worker's voting tendencies are set prior to the filing date), it would seem that a delay between the filing date and the election could not affect voting behavior.

The book raises a number of interesting questions that will inspire research in the future. One goal should be an integration of the basic economic logic of the situation with the political factors that Goldfield describes. I am troubled with the absence of an explicit discussion of the effect of unionization on wages, profits, and employment levels. The basic questions, which are relevant to the motivations and incentives of workers and employers, include How much do unions help workers (and hurt employers)? Is the assistance that unions provide worth more than the dues that unions charge? and Does the effect of union organization on business profitability depend on the type of industry and skill level of the work force? Confronting these issues would lead to a more realistic set of explanations linking employer resistance to the reduction in workers' willingness to vote for unions. For example, Goldfield mentions a study by Farber and Saks that found that within a workplace the higher-paid workers are more likely to oppose union representation. Unions equalize pay scales within workplaces, imposing costs on the higher-paid workers. Depending on the structure of the pay scale and the demographics of the particular work force, it can be that unions impose net costs on a majority of workers via the reallocation of salary. This type of economic result should be integrated into the explanation of union decline.

PAUL JOHNSON

University of Kansas

Consumers, Commissions, and Congress: Law, Theory, and the Federal Trade Commission, 1968–1985. Bernice Rothman Hasin (New Brunswick, NJ: Transaction Books, 1987. 236p. $39.95).

The Federal Trade Commission (FTC) provides a fascinating case study in bureaucratic politics. Responding to the consumer movement of the 1960s and 1970s, the "Little Old Lady of Pennsylvania Avenue" became a zealous regulator unafraid to take on the most powerful economic interests. In turn, however, the agency's aggressiveness precipitated an intense backlash in the late 1970s and early 1980s, and many of its policy initiatives were blunted or terminated as the result of legislative sanctions and personnel changes.

The FTC's policies and its political fortunes have received much attention from journalists, and part of its saga, such as its shift from "mailbag" to "structural" antitrust cases and its emphasis on rule making as a means of policing deception, have been dealt with at length by academicians. Hasin's work may be the first comprehensive treatment of the agency's volatile history over the past two decades, however, and as such it consolidates a good deal of information regarding key policy changes. Although her presentation sometimes lacks clarity and detail, the author generally does a good job in describing the grants of statutory power, the innovative legal theories in the areas of antitrust and unfairness and deception, and the new precedents in civil pro-

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cedure that provided the foundation for the FTC's activism.

Hasin also provides an explanation for the commission's behavior. She argues that the agency was captured by consumer advocates such as Ralph Nader and his minions, together with their allies on Capitol Hill. *Capture* may seem too strong a metaphor to many (as it does to me), and indeed the author fails to take on the argument made by Barry Weingast and others that the FTC has simply adapted to the changing balance of forces in its environment. Yet Hasin's assertion in the introduction that "consumerists" successfully established "network relationships within and external to government based on shared beliefs and common objectives" and that these networks enabled them "to influence and shape the direction and passage of law" (p. 2) is amply supported throughout the book. Her documentation of the FTC's persistence in the face of mounting conservative opposition in the mid-to-late 1970s further supports the capture thesis. Turning to ultimate causes, Hasin also examines the rise of consumerism itself. In so doing, she presents an interesting if limited discussion of the antiestablishment mood generated by Vietnam and the rise of a liberal and public-spirited "new class" to compete for political influence with the traditional bourgeoisie.

The book does have some limitations, however. The writing is awkward, both in terms of syntax and organization. In addition, Hasin's use of concepts from political science and economics sometimes reflects a lack of understanding or sophistication. For instance, her observation that "in one variant or another the capture theory of economic regulation is now predominant" (p. 1) may have been acceptable 10 or 15 years ago, but certainly not today. As another example, the employment of "issue networks" in support of her capture thesis seems to miss Heclo's main point.

Perhaps the most important flaw has to do with the author's treatment of consumerists, whom she views as arrogant, self-righteous, inflexible power seekers, ignorant of the needs and desires of the people they claim to represent. These charges may well be true to some extent, and they may help to explain the FTC's intransigence as political support for its activism dwindled. But by describing consumerists with such personal contempt the author detracts from what purports to be an objective analysis. Hasin's apparent admiration for regulatory reformers such as James Miller and her unsubstantiated endorsement of economic efficiency as a measure of sanity in policy-making have a similar effect.

All in all, however, *Consumers, Commissions, and Congress* is a worthwhile contribution that will serve as a valuable resource for students of the FTC and regulatory policy in general.

WILLIAM F. WEST

Texas A & M University

Leadership at the Fed. By Donald F. Kettl (New Haven: Yale University Press, 1986. xiii, 218p. $22.50).

In this volume, Donald F. Kettl provides an historical account of the Federal Reserve from its founding to the present, emphasizing the relationship between the president and the Federal Reserve chairman. According to Kettl, the Fed's "enormous" and "remarkable" power is a function of the degree of political support it receives from its constituencies (p. 9). Getting this support is the chairman's job. "It has been," Kettl writes, "the principal responsibility of the chairman to recognize the politically acceptable boundaries on [the Federal Reserve's] decisions; to keep those boundaries as broad as possible; and to attempt to keep the Fed's policies within those boundaries" (p. 13). The president is the most important constituent for the Federal Reserve, so the primary test of leadership for a Fed chairman is the maintenance of the relationship between himself and the president.

With the exception of a chapter devoted to Congress–Federal Reserve relations in the 1970s, the book focuses on the twists and turns in the relationship between the president and the Federal Reserve. Kettl identifies three kinds of patterns of relations (he also calls them *strategies of leadership)* between the Fed and president: accommodation, confrontation, and transformation. In periods of accommodation, presidents get the monetary policy they want. In periods of confrontation, there is conflict between the president and the Fed about whether to pursue economic growth or to fight inflation. Resolving these conflicts has re-

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quired fundamental redefinition of the Fed's mission and the chairman's relationship to the president. Such fundamental change involves transformation, the third kind of relationship, or strategy. Transformation is the distinctive result of "special leadership from unusual Fed chairmen" (p. 16)—Marriner Eccles, William McChesney Martin, and Paul Volcker. Kettl insists that "the driving power of these transformations came from the chairmen themselves" (p. 202), not the president.

The strength of the book is its historical scope and Kettl's skillful historical reporting. This is a lively chronological account of Fed-presidential relationships as revealed in interviews and presidential documents. The research involved is ambitious and impressive, drawing on documents from seven presidential libraries and at least three other specialized collections. As far as I know, no one writing on the Federal Reserve has taken such excellent advantage of the Burns papers in the Ford Library and the Erlichman papers in the Fenn Gallery in Santa Fe. These new sources will make chapter 5 a special treat for students of economic policy in the 1970s. Students of monetary policy and bureaucratic politics are certain to find the book useful as a rich historical compendium of documentation about a very significant aspect of monetary politics. No other single source provides such a complete survey of the important transactions between presidents and the Federal Reserve.

Kettl does not argue that his account reshapes our view of Federal Reserve history, nor does it appear to do so in any fundamental respects. There are, however, points on which Kettl offers new interpretations (e.g., on the reasons why Truman decided not to reappoint Eccles) and instances in which his account differs from those offered by his predecessors. As one example, consider the account of Marriner Eccles's conflict with Treasury Secretary Morgenthau in the late 1930s (pp. 55-59). Kettl, focusing on the leading role of Eccles, does not mention the hypothesis of Friedman and Schwartz (Milton Friedman and Anna J. Schwartz, *Monetary History of the United States, 1867-1960*, pp. 520, 522) that the policy conflict originated in a New York Federal Reserve Bank scheme to limit the power of the board. Had Kettl considered the Friedman and Schwartz view, it would have added a very interesting dimension of complexity to our understanding of the strategic situation. Clifford writes of these events that Morgenthau doubted that Eccles was the "master of his house" (A. Jerome Clifford, *The Independence of the Federal Reserve System*, p. 156), a position consistent with the Friedman and Schwartz account. Kettl concludes from his analysis that Eccles learned that "in any serious dispute with the Treasury, . . . he would have to fight the president, and that was one battle he could not win" (p. 56). However, Clifford, after examining many of the same primary sources, concluded that "the episode showed that the President would not always give quick support to the Treasury" (Clifford, p. 158). To have explored these links to prior scholarship would undoubtedly have made the work longer and less lively. It would also have added a valuable scholarly dimension.

Kettl's central argument that the decisive political relationship in this case is between the president and the chairman has been advanced by a number of other scholars. This is also true of Kettl's conclusion that the relationship is not primarily one of protracted, overt conflict. The novel information in Kettl's book mostly serves to add detail—often fascinating—to a larger outline already in place.

Readers concerned with developing concepts about leadership, exploring theories of bureaucratic power, and testing of hypotheses will find the book disappointing. There are many instances in the work when greater conceptual clarity, more skepticism about informant interview reports, and more analytic curiosity about the data would have produced improvements without loss of integrity in the historical mode of analysis.

In discussing the Fed's power, Kettl appears to conflate the Fed's bureaucratic power and the power of monetary policy to influence the economy. Monetary policy would be powerful in influencing the economy no matter who controlled it. But we would not call the Fed powerful without evidence of its bureaucratic power—its ability to achieve its goals despite political opposition. However, Kettl seems to regard a developing consensus among economists about the power of monetary policy to influence economic behavior as evidence of growing bureaucratic power at the Fed. More clarity about this might have led Kettl to place more weight on his evidence that presidents Wilson and Hoover both had considerable

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respect for the independence of the Federal Reserve Board long before the first "transformative" events of the 1930s (pp. 24, 38). The Federal Reserve Board's ability to pursue an apparently disastrous course of action during the Hoover years despite opposition from many quarters (pp. 29–44) testifies to its considerable bureaucratic power at an earlier date than Kettl seems to credit.

One would also welcome more systematic development of the sequential leadership patterns Kettl has discovered in this history. Does Kettl think his concept of *transformational leadership*, which is rather different from James McGregor Burns's similar-sounding idea of *transforming leadership*, can be applied to other settings? Can Kettl offer further guidance as to the hallmarks of transformational leadership and its dynamics? Is there a way of systematically characterizing a relationship so we can know when transformation is taking place?

The historical account of Fed-president relations does not always fit very well with the sequential leadership patterns Kettl describes. At least one of the three instances of confrontation Kettl identifies (1965–66) did not result in transformation. One of the three instances of transformation (1979–82) did not follow confrontation. Why does not Kettl consider the period leading up to the 1951 accord to be one of transformation? How was Chairman McCabe, a weak and mistrusted leader, able to pull off the accord? For that matter, why did Truman appoint a chairman he didn't trust? If the chairman was not, in fact, the central actor in this important—if not transforming—event, then perhaps the basic argument of the work needs modification. Perhaps that modification would be useful in understanding more fully why Arthur Burns, whose relations with Nixon were often strained, enjoyed much better relations with Ford.

On a minor level, I would note flaws such as these: Contrary to the opinion of one of Kettl's anonymous informants, the change from Martin to Burns did *not* result in more Federal Open Market Committee conflict, as measured by the frequency of split decisions with one or more dissenters (p. 120). The 1974–75 recession, which actually lasted from November 1973 to March 1975, was *not* one of the shortest in U.S. history (cf. p. 136). It is therefore not compelling evidence of the virtuous consequences flowing from close policy coordination between Arthur Burns and the White House. The data in Table 2 (p. 137) do *not* conclusively show that the CEA had a "steady bias for easy money." This is clear on Kettl's own evidence that the Fed itself has a "steady bias for tight money" and that the CEA has told presidents that Fed policy is "just right" nearly half the time.

Despite my complaints, I enjoyed reading this book and found myself engaged throughout. My desire that Kettl do more should not take away the fact that he has written a book both readable and useful. Students of macroeconomic policy will find it to be a valuable reference source.

JOHN T. WOOLLEY

University of California, Santa Barbara

Appointment of Judges: The Johnson Presidency. By Neil D. McFeeley (Austin: University of Texas Press, 1987. xi, 199p. $22.50).

Neal McFeeley guides us through the appointment process of federal judges from the perspective of the Oval Office during the Johnson administration. His is the sixth publication in a series of works based upon the presidential papers of Lyndon B. Johnson. As such, *Appointment of Judges: The Johnson Presidency* is a unique and authoritative description of the political pressures, strategies and proclivities that may enter the exercise of the presidential power to appoint federal judges. There are already some excellent studies of the politics of judicial recruitment and selection. That McFeeley further elucidates our understanding of the appointment process is to his credit and speaks to the importance of his study.

McFeeley begins with an overview of the appointment process that covers much familiar ground, yet integrates the major works in this area (notably those of Henry Abraham, Joseph Harris, and Joel Grossman). McFeeley paints for us a picture of a frustrated, almost aloof Lyndon Johnson prior to the 1964 election, supporting Kennedy appointees whom he does not enthusiastically endorse. Unable to fully develop his own appointment process, the president often delayed making nominations

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for long periods, and McFeeley blames most of the unqualified ratings of nominees attached to the Johnson administration on this period. After the resignation of Attorney General Kennedy and the 1964 election, the Johnson selection process began to emerge. From this point on, McFeeley hits stride in giving us the evidence to support his theme; namely, that Johnson's success in appointing "Great Society liberals" to the lower federal courts and the Supreme Court was the result of the selection process he designed to "balance the politics of judicial selection with his own policy objectives" (p. 138).

The success of the process depended upon several key elements, not the least of which was the president himself. Johnson's "values affected . . . the selection of judges to all levels of the federal judiciary" (pp. 51–52). Johnson's control over the process was substantially enhanced by another central ingredient, the almost one-man show in the Justice Department, Ramsey Clark. Clark and his liaisons in the White House were to ensure that after due respect was paid to senatorial courtesy no appointee slipped through the process who was not personally loyal to Johnson and politically committed to his programs.

Chapter 5, "The Criteria of Choice" (perhaps the best chapter), unveils "how" the Johnson machinery ensured the purity of the judicial candidates. The "loyalty program" was at its height in 1967, "rejecting those [judicial] candidates who were not 'all-out-J-men' and did not support Johnson's Vietnam actions" (p. 86). McFeeley attributes Carp and Rowland's findings that "appointees of 1967 were markedly less liberal than other Johnson appointees" to Johnson's insistence on Vietnam loyalty. This underscores not only Johnson's impact on the appointment process but also the relationship between appointing president and judicial behavior. McFeeley leads us further into this connection by using some of his more powerful documentation to illustrate how "rigorous" the screening was for judicial nominees with regard to their ideological loyalty to the Great Society, in particular, to civil rights. No appointment was made, particularly in the South, unless Ramsey Clark acknowledged that the nominee supported civil rights; "neutrality on the race issue [was] not enough" (p. 86). Many times President Johnson conducted personal interviews with nominees individually and in groups to ensure that they understood what he expected from them on this and economic issues.

McFeeley convincingly offers his analysis of the politics as an underlying explanation for the solidly liberal propensity of the Johnson appointees as evidenced in the major behavioral analyses. Johnson's concern for "correct" views on the civil rights issue does not come as a surprise. The intensity and duration of his commitment to the ideological purity of the nominees on the issue is, however, a revelation.

Finally, some of what McFeeley discusses is vintage Johnson humor. For example, Johnson would give a "good talking" on civil rights to the prospective nominees (sometimes appointees) and would also ask them to avoid getting "arrogant," to "act as though they had to stand for election every once in a while," and to "get off the bench when you reach retirement age. . . . There is something about the nature of the job with everybody saying 'Your Honor' which seems to cause this [arrogance]" (pp. 54–55, 169). Johnson's intense concern for appointment of younger judges, comparable in average age to Reagan's judicial appointees, was another successful selection strategy. "Seventeen years after leaving office, one of the Supreme Court justices, thirty-five of the judges of the courts of appeals, and eighty-seven of the district judges appointed by Johnson remain on the bench in active or senior status" (p. 4).

That "politics permeated the process of judicial selection in the Johnson administration" and that he "understood the political aspects of judicial selection" (p. 138) are understatements. Judgeships were poker chips in Lyndon Johnson's high-stakes political game. Only one caution to all those who shall read *Appointment of Judges*, don't slight the footnotes. Some intriguing information is there. The recruitment and selection of federal judges is an integral and significant aspect of the governmental process and judicial behavior—so much so that we can never have enough studies in the vein of McFeeley's work.

DEBORAH J. BARROW

Auburn University

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Parties in Transition: A Longitudinal Study of Party Elites and Party Supporters. By Warren E. Miller and M. Kent Jennings (New York: Russell Sage Foundation, 1987. xxv, 284p. $22.95).

Parties in Transition is a survey study of delegates to past conventions—specifically those of 1972, 1976, and 1980, in both parties—and it provides political scientists and journalists who are focusing on the 1988 campaign ahead with useful background on one key dimension of campaigns past. The book is about change and continuity in opinions, attitudes, and behavior of delegates as an important group of national campaign activists.

Warren Miller and Kent Jennings have drawn upon two large surveys of convention delegates. The first was done in 1972, using both personal interviews and mailed questionnaires; it has been reported on by Jeanne Kirkpatrick (*The New Presidential Elite*, 1976). The second survey, entirely mail questionnaires, was administered in 1981 to delegates to the three preceding conventions. A group of delegates interviewed in 1972 were thus reinterviewed, and new delegates to the 1976 and 1980 conventions were surveyed for the first time. The 1972 survey was first viewed as a stand-alone study of women in politics. In its aftermath, the principal investigators concluded that a panel design was needed, with the 1972 delegates to be reinterviewed after the 1976 and 1980 conventions. No further interviewing was done until 1981, however, and then the "new delegate" interviews (i.e., of those who were not delegates to the 1972 conventions) became of prime importance. All these shifts give an impression of the project's being disjointed and ad hoc. Nonetheless, interesting and important findings have been reached, and a wealth of data has been made available for secondary analysis through the Inter-University Consortium for Political and Social Research.

In one of the most valuable sections of their work, Miller and Jennings explore changes between 1972 and 1980 in the incentives for political involvement. Basically, they find that the new 1980 Republican and Democratic delegates differ not at all in their weighting of incentives for involvement—among those classified as *policy-oriented, partisanship, social,* and *personal career*. In 1972, however, the two delegate groups had differed greatly. The Democrats then responded more than the Republicans to policy incentives and much less to old-fashioned partisanship. The new 1980 GOP delegates were less moved by considerations of partisanship than their 1972 counterparts had been, while the new 1980 Democrats responded more to partisanship than had their 1972 brethren.

Miller and Jennings conclude that the several "classes" of Democratic delegates differ little in ideological orientation and policy preferences. The new 1980 delegates have much the same policy outlooks as do the 1972 class: "The delegates who nominated McGovern *and* who remained active in the 1980 campaign were, at least in 1981, not substantially to the left of their 1980 counterparts" (p. 135). The data bear this out. I disagree with the authors' conclusions about the Republicans, however. They argue that "those who were chosen as delegates in the year of Reagan's nomination [1980] were visibly more conservative than those first chosen in 1972 and 1976" (p. 133). In fact, the interclass differences Miller and Jennings present are far too modest to justify a "swing-to-the-Right" conclusion. The mean issue position score of the 1972 delegates was 71.1, that of the new 1980 delegates 75.2; the liberal-conservative self-placement of the former was 68.9, of the latter 74.4, on a scale where 0 is most liberal and 100 most conservative. In fact, neither partisan set of classes changed much in policy outlook; interparty difference, not intraparty change, is the big story.

Examining the differences between delegates and party rank and file, Miller and Jennings reach a conclusion that I find entirely persuasive: "Regardless of level of intensity, no rank-and-file stratum matches the least committed stratum of party delegates *in reflecting modal party positions*. . . . Since delegates are *committed activists*, it would be surprising if they did not exceed their 'constituents' in terms of ideological zeal" (p. 198, emphasis added). The authors go on, however, to argue that between 1972 and 1980, GOP elites swung to the Right, leaving them "seriously out of tune with their followers" (p. 219). In the earlier report on the 1972 survey we were told that Democratic delegates were then far to the Left and unrepresentative of their party's rank and file. Miller and Jennings are clearly

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uncomfortable with this conclusion and inferences drawn from it; elites always "exceed their 'constituents' in . . . ideological zeal." Still, they are inclined to fit their 1981 findings into the current popular theme that Reagan-era Republican elites have swung far right, compared to the public at large or Republican identifiers.

In fact, the data presented in *Parties in Transition* point to another conclusion. The "distance" between 1980 Reagan delegates (the most conservative group) and the Republican rank-and-file (from the 1980 Michigan election surveys) was 11 points on the liberal-conservative self-placement scale, and 20 points on the issues index (mean scores); while the distance between Kennedy delegates (the most liberal) and the Democratic rank-and-file was 20 points and 19 points respectively. The Carter delegates scored 49 in liberal-conservative self-placement compared to 55 for the party rank and file and 40 on the issues index compared to 49 for Democratic identifiers. Considering all measures, Democratic delegates in 1980 stood in about the same relationship to Democrat identifiers as Republican delegates did to GOP supporters in the general public—with the delegates in each case more ideologically coherent and distinct.

I should stress in conclusion that my differences with Miller and Jennings over the interpretations of their data pale in comparison to my appreciation of the overall strength of their study. They have given the profession a valuable collection of longitudinal data.

EVERETT CARLL LADD

University of Connecticut

The Limits of Victory: Ratification of the Panama Canal Treaties. By George D. Moffett III (Ithaca, NY: Cornell University Press, 1985. 261p. $24.95).

What are the prospects for presidential leadership if the biggest treaty ratification campaign in recent memory failed to budge public opinion? George Moffett analyzes ratification of the 1978 Panama Canal treaties as an "object lesson in the limits of public education" (p. 136). The degree that Jimmy Carter staked his presidency on these treaties is still remembered, as is the cost he paid during the longest hearings and debate in the Senate's history, to win with only one vote to spare ratification of a new Panama Canal Treaty and a Treaty of Permanent Neutrality. Moffett concludes that "it proved easier to arouse opinion than to direct it" (p. 131). This finding would be unremarkable had the Carter administration not devoted so very much time, effort, and scarce resources to the task.

Perhaps unusual in passionately dividing supporters and opponents, these treaties nonetheless illustrate "the difficulties involved in trying to force a major policy initiative on an unwilling public and a reluctant Congress" (p. 117). Moffett's book is a case study on this theme. It also has broader relevance to a recurring problem for U.S. foreign policy: the need to build a majority coalition in the absence of a majority. Thus the ratification campaign is a dramatic but not unusual case of how hard presidents and Congress must work to legitimate foreign policy.

The canal issue has passed, but deep divisions in the foreign policy consensus remain. Policy conflict continues, with where one stands depending on what one believes, as well as Allison's "where you sit." This insight goes back to early foreign policy theorists who accounted for decisions in terms of shared beliefs as well as organizational and vested interests. Complicating factors operating since the mid-1970s are fragmented and divisive—rather than shared—images and competing belief sets that cause conflict over foreign policy. Moffett, a historian, could benefit from the insights of foreign policy scholars who use a perceptual approach. Nonetheless, he does a good job, in the chapters "The End of Containment" and "The Divergence of Policy and Opinion," of setting canal policy in its historical and cognitive contexts.

Moffett poses three questions about the ratification campaign: 'Why did the treaties almost fail? How did the Carter administration finally win? and Why did the effort exact so high a price? Moffett is well equipped to suggest some answers. First as the research director for a protreaty citizens committee and then as assistant to the chief of staff in the Carter White House from 1978 to 1981, Moffett experienced the effort to mobilize support for the treaties. His participation enriches, and only occasionally taints, his analysis. Moffett interviewed over 50 participants in and outside of

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government and used additional oral history interviews now at the Carter Presidential Library.

The Limits of Victory has three sections. The first describes ratification strategy as planned and implemented. This narrative substantiates many of the points in previously published works (William L. Furlong and Margaret E. Scranton, *The Dynamics of Foreign Policy Making*, 1984; and William J. Jordan, *Panama Odyssey*, 1984). The "Organizing for Victory" chapter also adds new details and insights to the historical record that will delight those who follow canal policy. The episode that most analysts want clarified concerns why the White House accepted the DeConcini reservation. Moffett sheds some new light, letting participants give their own interpretations. But the sum of the parts is not a convincing whole. Moffett fails to assess responsibility, and leaves the reader unsatisfied in trying to comprehend a major blunder, if not a fatal flaw, in Carter's dealing with the Senate.

The most original part of the book is his analysis of public opinion. Despite the impression at the time of protreaty movement in national polls, Moffett found that public opinion "remained almost completely static, . . . public attitudes never budged" (p. 116). Moffett used long-term trend analysis to examine comparable questions in different polls and to compare questions grouped by information content. For those interested in the data, Appendix A contains all 36 questions asked by 10 major organizations between 1976 and 1978. Moffett concludes that apparent movement "was nothing more than a trend toward wording in the polling questions themselves," with the wording itself becoming "more likely to draw favorable responses" (p. 122). However, changes in wording reflected exactly what the Senate was doing at the time: clarifying and strengthening treaty language on U.S. canal defense rights. Moffett's findings do not diverge from those reported by the State Department's opinion analyst, Bernard Roshco ("The Polls: Polling on Panama—Si; Don't Know; Hell, No!" *Public Opinion Quarterly* 42 [1978]: 551–62). Moffett's conclusions are confirmed in large part, despite some methodological criticism, by Ted J. Smith III and J. Michael Hogan ("Public Opinion and the Panama Canal Treaties of 1977," *Public Opinion Quarterly* 51 [1987]: 5–30).

Of broader importance is the problem that Moffett demonstrates. To what extent is such an opinion pattern an artifact or a measure of reality? Moffett sees an artifact and rightly chides the press for overgeneralizing a pattern based on an artificially high baseline for anti-treaty opinion. That is a fair point. But shouldn't poll questions change as policy content changes? If we want to know public reactions as policy evolves in the making, don't pollsters have to include new details as they emerge?

In the third part of the book, on interest groups and ratification, Moffett almost lapses into a plaintive mode chastising affected interests for not doing more to help the administration. In "A Policy without a Constituency," he describes the activities of groups ranging from the United Nations Association, the UAW, and a variety of church organizations to banks and corporations. This is the weakest part of the book, largely because groups were, in fact, so peripheral to the ratification effort. In "The Economics of Ratification," Moffett analyzes lobbying by those most directly affected by the treaties, U.S. flagship owners, public port authorities, and midwestern farm exporters. As Moffett rightly notes, the adverse impact on administration plans by these maritime interests was exacted after the treaties passed, in the implementing legislation that had to pass both houses.

All in all, Moffett's book fills a gap in the literature on the 1978 canal treaties. He analyzes all of the domestic political influences on the ratification process and very ably tells the story of the administration's ratification plans and its "Pyrrhic victory" (p. 11).

MARGARET E. SCRANTON

University of Arkansas, Little Rock

The Political Theory of the Compound Republic: Designing the American Experiment. By Vincent Ostrom. 2d ed. (Lincoln: University of Nebraska Press, 1987. xxviii, 240p. $22.50 cloth; $8.95 paper).

In *The Political Theory of the Compound Republic*, Vincent Ostrom seeks to reconstruct the philosophical assumptions that underlie our original form of government and to

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demonstrate their continuing relevance and practicability. At the core of Ostrom's analysis is the notion of "constitutional choice." Beginning with the Declaration of Independence, Ostrom emphasizes the extraordinary degree to which the ex-colonists were free to design institutions which would secure their "safety and happiness." Although choice is always circumscribed by political necessity, necessity for Ostrom turns out to be rather benign. There is no mention of slavery here; necessity, in the form of the existence of independent political communities, points happily to some form of compound republic. By a *compound republic* Ostrom means not only a federal system of government but also "compound of decision structures within each unit of government" (p. 152). Still, because federalism is the most novel contribution to the U.S. experiment, Ostrom rightly devotes most of his attention to this principle.

In elaboring the role of federalism and its place in the compound republic, Ostrom relies principally upon the arguments of James Madison and Alexander Hamilton in *The Federalist.* In so doing, he explicitly rejects the interpretation of Martin Diamond and others that Publius, far from embracing federalism as a political good, remained skeptical of this innovation. He seeks to explain the discrepancy between Madison's and Hamilton's known antagonism to the states at the Federal Convention and their newly acquired "zeal for the federal principle" (p. 107) by suggesting that they learned something from their deliberations in Philadelphia. Although this is certainly possible, Ostrom offers no evidence in support of his claim.

To separate himself further from earlier theoretical studies, Ostrom disclaims any interest in the *motives* (which he mistakenly believes to be the same as the *original intentions*) of these two framers. This is partly because motives (unlike intentions) can never be known with certainty, but it is also because he wants to examine the idea of federalism strictly on its merits. Yet so congenial does Ostrom find the idea of federalism that he cannot bring himself to believe that Madison and Hamilton did not share his view. Despite his announced intention, Ostrom repeatedly brings his "sympathetic" reading of *The Federalist*'s view of federalism to bear on the merits of the principle.

Chapters 2–7 seek to "reconstruct" the theory of the compound republic as it emerges in *The Federalist* and to relate this theory to the larger issue of constitutional choice. Here Ostrom argues that the political theory of *The Federalist* provides a genuine alternative to the Hobbesian view of the modern state. Yet once we get past the obvious differences, Ostrom's reading of *The Federalist* suggests a far closer affinity with Hobbes than is in fact the case. According to Ostrom, *The Federalist*'s view of human nature is one of selfish individualism, while the purpose of government is the efficient allocation of goods and services. In a work that seeks to recover the political theory of the compound *republic,* it is surprising to find not one trace of republican thought. There is not the slightest suggestion that *The Federalist* prized republican self-assertiveness or saw the continuing necessity of civic virtue, even if it no longer relied exclusively upon it. Ostrom's "basis assumptions" are rather the building blocks of liberal political philosophy, as first articulated by Hobbes and later refined by Locke and Hume.

As a political theorist, I found Ostrom's discussion of the practical question, How well has the compound republic worked? to be the most engaging. Here Ostrom tackles two big questions, slavery and the rise of machine politics and boss rule. I admire his forthrightness in raising the slavery issue in connection with federalism but was disappointed in his treatment. Ostrom fails to consider the role of the states as political units in perpetuating slavery and racial discrimination. Instead, he focuses on the "variety of remedies" presently available to blacks in the compound republic.

Ostrom then discusses how the federal system was corrupted after the Civil War and transformed by Progressives into a centralized national government. But in contrast to those who continue to press for greater centralization and "responsible" government, Ostrom insists that a return to the original compound republic can promote both justice and efficiency. These are big claims, and the reader is naturally led to wonder how a federal system will produce all these political goods. At this point, Ostrom moves to a discussion of a "federal theory of public administration," but his argument needs further elaboration to be made comprehensible to the generalist. If, in the last chapter, Ostrom had developed his

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federal theory of public administration instead of recounting his intellectual odyssey through, among other things, "Laswellese" and "Economese," the practical advantages of the compound republic, so badly in need of restating, might have emerged more concretely.

JEAN YARBROUGH

Loyola University of Chicago

When Federalism Works. By Paul E. Peterson, Barry G. Rabe, and Kenneth K. Wong (Washington: Brookings, 1986. xvi, 245p. $28.95 cloth; $10.95 paper).

In the years since the original publication of Morton Grodzins' classic, *The American System*, a growing chorus of critics has charged that the "mild chaos" that Grodzins praised has reached the point of total chaos, in which confusion and complexity have rendered the federal system totally unmanageable. In a valuable reassessment, Peterson, Rabe, and Wong present an in-depth analysis of a number of federal domestic programs which, in their early years, contributed to the image of total chaos. The authors find, however, that persistence, growing professionalism of local administrators, and some additional flexibility from national officials often succeed in overcoming the implementation problems that commonly face new national-local programs.

The authors contend that the image of cooperative federalism emerged from the federal system's early emphasis on *developmental* programs, which seek to boost individual local economies. The general agreement on goals led to relatively harmonious relationships among national, state, and local governments. In the 1960s and early 1970s, more emphasis was placed on *redistributive* programs, which seek to reallocate wealth (or other things of value) from one group to another. The controversial nature of redistributive programs provoked considerable difficulty in their early years, a circumstance that led to charges that the federal system had grown unmanageable. In time, however, the difficulties were overcome to a large degree.

In spite of the relatively cooperative nature of developmental programs, the authors conclude that they generally involve activities that state and local officials would support anyway (assuming that they have sufficient resources, a matter of some uncertainty). As a result, a national role in developmental programs may not be necessary in most cases. By contrast, although a national role in redistributive programs is often controversial, interjurisdictional competition for wealth and jobs makes subnational governments biased against redistributive efforts. A national role is therefore necessary for redistributive programs. The frequently hostile local political climate often means that redistributive programs are more effectively administered by professionals who are somewhat insulated from the local political environment. The local appeal of developmental programs, however, causes politicized administration of those programs to be relatively successful. A mayor who believes his or her political career will be boosted by a downtown redevelopment project can be strikingly effective in overcoming obstacles to that project. Administrative arrangements that are suitable for one type of program, then, may be highly unsuitable for another.

The most problematic part of the analysis is the recommendations for reform—always a perilous enterprise in U.S. federalism. The recommendations stem from the following premise: "In general terms the proper distribution of power (in a federal system) can be readily stated" (p. 11). Decisions that have very broad consequences should be made nationally; decisions with localized consequences are best made locally. The history of U.S. federalism indicates that this formulation is extremely difficult to apply in practice. Were northern whites affected by discrimination against southern blacks? Does vocational education have national consequences? The authors' answer to the latter question is negative; however, in a society with a mobile labor force and significant foreign economic competition, the cumulative effects of a number of local vocational education programs might have significant national consequences. As Schattschneider argued in *The Semisovereign People*, the size of a political conflict often has major policy implications; making decisions regarding the distribution of responsibilities (national, state or local or both) is, consequently, rarely straightforward.

The authors contend that developmental policies normally involve activities that state

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and local governments are inclined to support with or without national help and that national grant programs that support developmental activities are not very effective in targeting resources to jurisdictions with the greatest needs. As a result, the national government should avoid involvement in developmental programs unless significant externalities are present or a redistributive program needs some developmental features to enhance its acceptability. The national government would then be free to devote more attention to redistributive programs, a shift that might lead to improvements in individual redistributive programs and to greater coordination across redistributive programs.

This formulation presents several problems. First, it asks the president and Congress to refrain from involvement in programs that are often popular and build political goodwill and to focus their attention on programs that are typically controversial and create political antagonisms. That advice can expect a cool reception in Washington. Moreover, to the degree that developmental programs help to create political obligations that can help build support for decisions on other issues, an end to national involvement in developmental programs could lead to even greater difficulty in dealing with redistributive issues. Finally, national developmental programs that manage to provide *some* resources to localities in need (at the expense of providing resources to areas not in great need) may still have merit if the alternative is nothing for areas in need. When zoning, building codes, and other devices are used to exclude poor people from many localities, aid to people and aid to places often overlap, particularly if policymakers want poor people to have opportunities in the private labor market.

Overall, *When Federalism Works* is a valuable corrective to the view that the federal system is an incomprehensible, unmanageable morass. Students of intergovernmental relations, public policy, and public administration would do well to explore its insights.

DAVID C. NICE

University of Georgia

Controlling the Federal Bureaucracy. By Dennis D. Riley (Philadelphia: Temple University Press, 1987. xiv, 200p. $24.95).

Most of the scholarly work on accountability and control in public administration derives from the well-known Friedrich-Finer debates on internal versus external sources of control. This book addresses the latter type of control. It describes the main institutional sources of external control over the federal bureaucracy, namely, the president, Congress, the courts and various components of the broader public. The book's main thesis is that the well-known combination of special interest groups, congressional committees, and career public employees (whether iron triangles, subgovernments, or issue networks) results in a policymaking process that makes it difficult to control the federal bureaucracy. The author moves from this widely accepted characterization of U.S. public administration to a much more controversial one about the federal bureaucracy, namely, that the federal bureaucracy is fundamentally irresponsible.

The book is well written and presents very thorough explanations of the subtleties of various external control mechanisms and details the nuances of controllers' interactions with public administrators. In the process of detailing control strategies, the book illustrates some fundamental dynamics of public administration, such as how an accumulation of several reasonable, independent administrative decisions can have unintended consequences. The book's section on law as a control mechanism presents insights that are often missing in discussions of control of public administrators.

The book has two serious shortcomings: its narrow focus leaves us with an incomplete picture of the prospects for accountability of public administrators, and it puts forward a conclusion that is beyond the reach of the data presented. Of course, choosing to limit the scope of his book to external mechanisms of control is the prerogative of the author. However, the book is not exhaustive in its discussion of external sources of control. For example, there is no discussion of the media as watchdogs with potential to contribute to external control efforts. Of more serious concern, the book is incomplete because it does not address adequately the issue of cumulative

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or interaction effects of the various control mechanisms.

Having once made the choice to limit the scope of his analysis, the author must also accept limitations on the breadth of his generalizations. The conclusion of this book, namely, that the federal bureaucracy is fundamentally irresponsible, cannot be reached from the data and discussion presented in the book. The author only makes passing reference to internal mechanisms of accountability. Hence we know only part of the accountability story. More importantly, the book does not present analyses or systematic arguments to show that external mechanisms fail to result in responsible federal administrators. Furthermore, failures at external control do not *necessarily* yield irresponsible administrative behavior. To support this conclusion the book would have to examine the cumulative effects of the various mechanisms of control, rather than just the potential for their separate failures, and link those effects to irresponsible administration. It may be possible to make such a case, but the author has not done so in this book.

It is time to move beyond the Friedrich-Finer debate and the external-internal dichotomy of accountability. Accountability is a complex, dynamic *relationship* between administrators and a multitude of diverse, legitimate claimants. Agencies and administrators play an active part in the accountability relationship. They often take the initiative in meeting claimants' expectations and frequently can choose from a variety of public sector accountability mechanisms. Such administrative initiatives can yield both responsible and irresponsible behaviors. Consequently, complete models of accountability must incorporate the perspective of public administrators. We need to explore both the opportunities and constraints public administrators see in the various accountability mechanisms and the conditions under which administrators are likely to rely on one mechanism over another.

The book presents very good descriptions of external control techniques. We know that public administrators often face different types of accountability mechanisms, sometimes simultaneously, at other times in sequence. Some of these types of accountability systems are discussed here, especially the political and legal mechanisms. Other accountability mechanisms, such as bureaucratic and professional systems and the effects of these systems on administrative behaviors, are missing from the discussion. Given these omissions, the book cannot support the conclusion it offers to its readers.

BARBARA S. ROMZEK

University of Kansas

Braking the Special Interests: Trucking Deregulation and the Politics of Policy Reform. By Dorothy Robyn (Chicago: University of Chicago Press, 1987. ix, 295p. $24.95).

Of all the deregulatory successes of the last two decades, the reform of the motor carrier industry is perhaps the most exceptional. It represents a rare victory for diffuse political forces over concentrated interests. Dorothy Robyn furnishes an eyewitness account of the final months of the saga that led to the passage of the Motor Carrier Act of 1980.

Robyn maintains that four "ingredients" explain the changes in the regulatory system: (1) strategic use of economic evidence and analysis to demonstrate the merits of reform; (2) formation and maintenance of an ad hoc coalition to lobby actively; (3) use of transition strategies to soften the opposition to change; and (4) strategic bargaining by the president to gain sheer political leverage. After a brief survey of motor freight regulation and an overview of the political battle over trucking deregulation, the author analyzes each of these four factors in turn. She relies heavily on her interactions in Washington with those involved in the reform effort—particularly the members of the ad hoc interest-group coalition favoring change—in the eight-month period that culminated in the signing of the Motor Carrier Act. Robyn concludes by buttressing her argument that these four elements of political strategy were necessary and by indicting economic theories of utility maximization.

The strength of this book is that it provides fascinating details of the machinations that took place before legislative victory was achieved. However, the fact that the author only went to Washington in December 1979 may have had some negative consequences. In

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particular, Robyn pays relatively little attention to pre-1979 events at the Interstate Commerce Commission (ICC) that substantially altered the regulatory system. Generally, her analysis of the ICC is less thorough than her exploration of the agency's external political environment. This discrepancy probably reflects her involvement in the ad hoc pro-reform coalition that by late 1979 was pushing for statutory passage of deregulation. Despite these reservations, the author provides a level of detail that will undoubtedly be very satisfying to those fascinated by deregulation or how large-scale policy changes are possible in a politically charged context.

The book is weaker in its attempts at developing a theoretical framework. To an extent, such objections are endemic to any case study. Yet not all of the ingredients stipulated as necessary for reform were present even in the motor carrier case. Consider that evidence on the costs of motor freight regulation existed long before motor carrier reform occurred; that the ad hoc interest-group alliance was no match for the American Trucking Association and the Teamsters; and that transition strategies were not heavily employed. Nor is it apparent what tangible instruments Jimmy Carter had at his disposal, besides his own persuasive abilities, that made him more successful than his predecessors. These shortfalls suggest that Robyn has not delineated all those factors that were sufficient for change.

This leaves one with the feeling that perhaps the author was too quick to reject economic theories of politics. Clearly, Robyn is correct in her assertion that explaining deregulation is problematic for those adhering to the standard economic perspective on regulatory politics. Nevertheless, the analysis of reform requires a more intensive investigation of political actors' incentives and resources. If theories are going to be rejected, then they should be given a thorough hearing.

Robyn's work will be of interest to anyone concerned about deregulation or nonincremental policy change. Whatever its shortcomings, it chronicles an important segment in the history of economic regulation.

LAWRENCE S. ROTHENBERG

California Institute of Technology

Organization in a Changing Environment: Unionization of Welfare Employees. By Russell K. Schutt (Albany: State University of New York Press, 1986. xiv, 243p. $34.50 cloth; $14.95 paper).

Observers of welfare politics sometimes speak, almost yearningly, of the prospects for uniting welfare workers and their clients in behalf of social reform. That prospect seemed tantalizingly close during the midsixties, when demands for reform came both from clients on the receiving end of the welfare system and also from the bureaucrats within the system. In the politically charged atmosphere of that era, clients mobilized within welfare rights organizations and bureaucrats mobilized within labor unions briefly raised the possibility of a two-pronged attack on the welfare system.

Robert K. Schutt's account of workers' unions suggests that hopes for a worker-client alliance were, and may continue to be, nothing more than wishful thinking. This book indicates that bureaucratic activism was an epiphenomenon associated with the temporary infusion into the welfare bureaucracy of a small cohort of college-educated activists who did not share the long-term career interests or short-term bread-and-butter concerns of their colleagues in the permanent bureaucracy. Schutt's study of welfare-worker unionism provides a valuable counterpoint to the literature on the rise and fall of client-centered organizations. His detailed and well-documented case study of welfare-worker unionization in Cook County, Illinois, portrays a radical union movement born with the fervor of social reformism that quickly shifted from "social consciousness" to "bread-and-butter" concerns.

Schutt attempts to account for these and other changes that occurred in the welfare workers' union from its inception in 1965 to the early 1980s. He also seeks to elaborate an analytical model of organizational change that incorporates both internal and external developmental influences. The model Schutt proposes focuses attention on changes in the sociopolitical environment that influence an organization's goals, the involvement of its members, and its structure and tactics. Many of the elements of this model will be familiar to students of organizations. However, the

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framework that Schutt specifies seems to offer few new insights into the political sociology of organizational development and, at times, fails fully to appreciate the richness of the existing literature in this field.

The real value of this book is to be found in the case study that traces the formation and evolution of the Illinois Union of Social Service Employees. In an enterprising use of multiple methodologies, Schutt, a sociologist, pieces together this story using data from interviews, quantitative survey research, and archival histories.

The union began as the Independent Union of Public Aid Employees, whose members "were just an informal group that got bigger," as one early activist recalls (p. 52). The so-called militants that formed the union were largely comprised of new employees, many of them recent college graduates infused with both the alienation and idealism of the sixties. They pursued conventional union demands for formal recognition of workers' rights but also less conventional demands for institutional responsiveness to the needs of poor families.

These dual concerns were linked in an early statement of goals and principles in which union organizers asserted that "a stable, experienced and trained staff, operating under conditions which permit them to exercize *[sic]* their competence, is prerequisite to the ability of the department to accomplish its stated purpose" (p. 54). They concluded, in a rhetorical flourish, that "should this be the end result of our efforts, we will have witnessed the creation of a new weapon in the increasing struggle for the commonweal" (p. 54).

But in the short space of a year, concern for the "commonweal" had taken a back seat to concern for the survival and growth of the union itself. Union leaders, needing the support of rank-and-file welfare bureaucrats, turned to the "bread-and-butter" issues that directly motivated their constituents. Generally, Schutt finds that the ebb and flow of union membership depended largely on whether workers believed that the union could deliver better wages and working conditions. Rank-and-file members were unimpressed, if not hostile, toward efforts of "militants" to join clients in protesting welfare policies. In this context, the concern for the "commonweal" that motivated some bureaucratic activists in the sixties necessarily took a back seat.

Here Schutt touches on a theme of central interest to students of social movements and community organizations. As scholars (notably William Gamson, Michael Lipsky, Francis Fox Piven, and Richard Cloward) have documented in a variety of contexts, organizations advancing social causes are often torn between the conflicting demands of institution building and the pursuit of their fundamental political objectives. Apparently, the union of welfare workers in Illinois, like its organizational counterparts elsewhere, resolved that conflict in favor of institution building and conventional union politics.

While Schutt does not always use his data to their full advantage in addressing some of the themes noted here, his case study does provide a valuable glimpse into the activities and motivations of workers in a significant public bureaucracy. For those sufficiently interested to delve into the case data, this book offers a useful perspective on the complexities and tensions that produced, altered, and ultimately attentuated union activism within the welfare bureaucracy.

EVELYN Z. BRODKIN

University of Chicago

The Burger Years: Rights and Wrongs in the Supreme Court, 1969–1986. Edited by Herman Schwartz (New York: Viking, 1987. xxv, 292p. $22.95).

If one enjoys well-written liberal critiques of conservative Supreme Court policy, this volume is recommended reading. It contains an introductory essay by the editor, one chapter on the court access decisions of the Burger Court, four chapters on First Amendment rights, three on equality, three on criminal justice, and four on economic regulation. Some of the pieces are written by leading authorities: Yale Kamisar on criminal procedure, Morton H. Halperin on national security and presidential power, and Michael Meltsner on capital punishment, to name a few. Virtually all chapters conclude that the Supreme Court wrongly decided many significant questions of civil liberties and rights between 1969 and 1986. The only contributor who has much to say commending the Burger

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Court is Wendy W. Williams (law professor at Georgetown University), who writes the chapter on sex discrimination.

The book's stated purpose is to examine the "legal soundness" and "real-world impact" of the Burger Court in the selected topics. Additionally, most chapters contrast to a limited extent the policy trends of the Warren and Burger courts and make some general predictions concerning the Rehnquist Court. As would be expected, they typically conclude that during the 17 years under Chief Justice Burger, major strands of Warren Court policy tended to survive—although at times severely battered or altered. The two areas of legal policy identified as experiencing the most dramatic shift to the Right are access to federal courts and national security. As for the Rehnquist Court, the volume's contributors almost always suggest that the nation can expect a continuation of Burger Court trends, with more conservative decisions on some substantive issues.

Generally speaking, the legal analysis of case law and decisional trends is of high quality. Sometimes much more could have been said about Burger Court decisions; the editor undoubtedly imposed fairly strict page limitations on the contributors' manuscripts. Yet one need not have a thorough knowledge of public law or Burger Court trends to understand the volume and to learn about the court's doctrinal tendencies during this period of its history. And unlike many edited collections, *The Burger Years* has coherence as a unit.

Beyond these considerations, the book contains other features that should be of interest to political scientists. For example, it contains a number of references to the policy preferences of the Reagan administration, with suggestions of the extent to which Burger Court decisions were consonant with Reagan's political and legal leanings. Thus, the chapter on separation of church and state by Norman Redlich (dean of the School of Law, New York University) and the chapter on racial discrimination by Haywood Burns (dean for urban and legal programs, City College of New York) will make informative reading for those concerned with the issues of school prayers or affirmative action.

Beware again, however, that this volume reflects somewhat limited scholarly objectivity. Even those with liberal views may question the accuracy of some of its analysis and conclusions. Although some of the chapters are quite objective, such as the one by Redlich, others are not. To a large extent, the book's subjective tone is a function of those selected to contribute to it. Most contributors have held academic posts at one time or another but are principally known for their active involvement in reform groups, research organizations, public interest law firms, and the like. Such a line-up predictably has its pluses and minuses. The book makes a valuable contribution to the literature on the Burger Court but must be viewed chiefly as a liberal assessment of a conservative era in Supreme Court history.

CHARLES M. LAMB

State University of New York, Buffalo

The Right to Life Movement and Third Party Politics. By Robert J. Spitzer (New York: Greenwood, 1987. xii, 154p. $29.95).

This slim volume makes a substantial point. Like Rosenstone and colleagues' *Third Parties in America*, Spitzer demonstrates that third parties are not an oddity in U.S. politics but a fairly regular response to failures of the major parties to provide responsive and capable leadership. The book focuses on single-issue parties and, in particular, the New York-based Right to Life party (RTLP).

Spitzer attempts to do four things: First, he examines four single-issue parties of the nineteenth century (Liberty, Free Soil, Greenback, and Prohibition), in order to generalize about the characteristics of single-issue electoral movements. Next he offers what he terms a "dynamic model of third parties" in general, not limited to single-issue parties. Third, he details the development of New York's RTLP in the context of that state's electoral rules. And finally, Spitzer argues that a reform of the national two-party system to resemble New York's—a kind of multiparty system with two parties dominant—would stimulate and strengthen U.S. politics.

One of the most interesting parts of the book is that it provides a fine example of the way electoral rules shape the development of political institutions. The rule in question is New York's cross-endorsement provision. Minor parties able to win a position on the

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state ballot are permitted to slate candidates who are already running on another party's line. Because a candidate's votes on the minor party ticket are added to his or her major party vote to produce a final count, casting a third-party ballot for a cross-endorsed candidate is not "wasting" a vote. The rule, Spitzer argues, removes a major constraint on third-party voting, helps sustain minor parties, increases their value to many candidates, and expands their influence on the major parties' choice of candidates and issues.

But the book comes closer to being four separate essays whose relationship to one another is not as clear as it should be, than to developing a coherent argument. The author's stated concern is with third-party politics, and that is the subject of his "dynamic model." His case studies, however, involve a subset of third parties: single-issue parties. His reasoning is that single-issue parties are a kind of limiting case: that they are more troublesome and disruptive than other types of minor parties—because they are led by zealots, take extreme positions, and view compromise the way Dracula reacts to the cross—and therefore that if even these troublesome single-issue parties can be shown to have positive effects for the party system, the case for the value of a multiparty system is enhanced. These very characteristics of single-issue parties, however, make them unique among minor parties and also may threaten both their effectiveness and their longevity. Their use as a limiting case, then, needs more justification. The ways in which they resemble, and differ from, other kinds of minor parties should be more fully specified.

The author's plea for the liberation of U.S. politics from "two-partyism," while most intriguing, would also be strengthened by closer connection with the rest of the book. Spitzer contends that the current major parties, while inadequate in many ways, have nevertheless developed a stranglehold on U.S. politics that has prevented the long-awaited (and, he writes, much-needed) realignment from taking place. A set of electoral rules that—like New York's—encouraged third parties could reinvigorate the party system. Reacting to the charge that third parties threaten political stability, Spitzer suggests that "a party system is principally a reflection of the nation's society" (p. 124) and that the U.S. two-party tradition will prevent the fragmentation and instability of some multiparty systems. But this reassurance seems to gloss over the author's own deft illustration, earlier in the book, of the very real impact of electoral rules on political behavior.

The data analysis presented—a survey of RTLP leaders and activists—is quite limited. Further, readers expecting an extended discussion of the abortion issue and the national pro-life movement will find only a brief section; the book concentrates on New York and the RTLP. But these lacks shouldn't erase the book's contribution. As an argument it needs to be fleshed out much more fully. Still, it offers an interesting challenge to our thinking about the impact of a two-party system on the vitality of U.S. politics.

MARJORIE R. HERSHEY

Indiana University

Religion and Politics in the United States. By Kenneth D. Wald (New York: St. Martin's, 1987. xiv, 301p. $29.95 cloth; $10.00 paper).

In *Religion and Politics in the United States,* Kenneth D. Wald describes a tense conversation with a fellow political scientist about current religious issues that abruptly ended when his colleague declared, "I *hate* religion and politics!" (p. 270). Wald includes the anecdote as an illustration of how the moral and personal dimensions of religion in politics can undermine any semblance of scholarly discourse. The incident also underscores the need for the balanced, carefully crafted text that he has produced for the emerging subfield of religion and U.S. politics.

Wald begins with a documentation and analysis of the persistence of religion in U.S. life as evidence that religion deserves more attention than it has received in the social sciences. (See also J. David Fairbanks, "Religion and Morality in American Political Life," *PS* 19 [1986], 869–71.) This is followed by chapters on the incentives and opportunities available for religious participation in politics, Puritan contributions to U.S. political culture, Supreme Court resolutions to church-state conflicts, four chapters on different aspects of religion and U.S. public policy, and his conclusions. The broad scope of the book is

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supplemented by a lengthy list of references in each chapter attesting to Wald's thorough secondary research. Because it is well organized with useful summaries, the narrative avoids an encyclopedic style.

A primary objective of the book is to dispel the scholarly stereotype "that religion is inevitably a force for conservatism and preservation of the existing social order" (p. xii). Wald accomplishes this goal most effectively in the policy chapters where he first examines General Social Survey data on the political identification and policy positions of six religious groupings. One finding from this exercise is that gains in social status were associated with a shift toward Republicanism for white Protestants; however, gains in status had less effect on prompting conservatism among Catholics and practically no such effect on Jews, black Protestants, and persons without religious affiliation. The author is careful to note that religious affiliation is one independent variable among many; yet he also asserts that relationships with policy positions cannot be dismissed as spurious.

Wald continues his religious diversity theme in two contrasting chapters addressing the mobilization of politically conservative evangelicals and the shift of Catholics and mainline Protestant clergy toward political liberalism. In the latter chapter, one glaring omission is any mention of liberation theology, a theology that is challenging Protestants as well as Catholics to become politically active on behalf of the oppressed. If Wald wishes to demonstrate that religion in politics does not always support the status quo, then discussion of the radical teachings of liberation theology seems warranted. Such gaps are perhaps inevitable in a survey text such as this (for instance, Wald's treatment of the complexities of Puritan covenant theology is questionable in its representation of a God bound by contract).

The author is to be commended for tackling the controversial issue of whether religious activism is beneficial or detrimental for our political system. He critiques numerous attitudinal studies dealing with religion and decides that "the evidence supporting a link between religiosity and the propensity to intolerance is not strong enough to warrant alarm" (p. 272), a decision based on mixed findings in the research. The more sophisticated studies focus on the *quality* of religious involvement, noting a distinction between those who rigidly follow religious authority and those who have internalized ethical principles of religion into their own attitudes and behavior. Wald reports these findings; but he has no ax to grind regarding the role of religion in politics, and he also cites historical and current examples of religious and political fanaticism. His neutral conclusion is that "an upsurge in religiously based political activity is neither to be welcomed uncritically nor condemned out of hand" (p. 285). Throughout this valuable text, Wald's sensitive, even-handed approach leaves one questioning why political scientists have so long avoided this important topic.

ANNE MOTLEY HALLUM

Stetson University

Philosophy, "The Federalist," and the Constitution. By Morton White (New York: Oxford University Press, 1987. xi, 273p. $29.95).

Presented as the sequel to his influential 1978 *The Philosophy of the American Revolution,* Morton White offers a rigorous, philosophic examination of the main arguments contained in *The Federalist.* Although *The Federalist,* its language and internal logic, is the central focus of White's attention, he skillfully draws on other writings by John Jay, Alexander Hamilton, and James Madison to clarify his position when necessary. White argues that Publius can be understood more fully by reading *The Federalist* "with one strict philosophic eye on the text of Publius and the other on any philosophic text which influenced him when he was writing" (p. 8); the latter approach is particularly relevant since the authors "frequently used philosophic terms such as 'reason,' 'human nature,' 'science,' . . . 'duty,' 'good,' 'passion,' and 'interest' without saying much about *how* they were using them" (p. 4). Most prior scholarship, White claims, has appreciated the role of philosophy in *The Federalist* only in the excessively narrow terms of Publius's theory of human nature or his view of history, failing to recognize that "it contains or relies upon a theory of knowledge, a doctrine of normative ethics, a psychology of motivation, a theory of the causes of

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faction . . . and even some metaphysics and theology" (p. 7). This book, then, is a careful, analytic exegesis of the complete philosophy contained in *The Federalist*.

White presents an exhaustive explanation of the relevance of both David Hume and John Locke, "the most important philosophers," to Publius: Hume shaped the authors' "methodology or epistemology of political science," Locke their "ethics and theory of ethical knowledge" (pp. 9, 20, 22). In the process, White engages the seminal work of Douglass Adair and challenges the interpretations of Charles Beard, Arthur O. Lovejoy, and Robert Dahl. The discussion of Adair's understanding of Hume's influence on Madison is particularly instructive; the lengthy analysis of Beard and Dahl, however, borders on being superfluous. White's objections to Beard and Dahl are not in error, but they add little to the contemporary scholarly debate on understanding Publius. One wishes that this talented thinker had devoted his attention to discussing the more recent interpretive efforts generated by Bernard Bailyn, J. G. A. Pocock, and others.

In his discussion of Madison's theory of human motivation, White articulates the significant differences between Hume's and Madison's views of factions. While Madison believed all economic interests constituted factions, Hume denied that "the *landed* and *trading* part" of England were factions; similarly, Hume argued that "a skillful legislator" could prevent the rise of factions while Madison "thought that factions based on interest are not preventable in civilized nations" (p. 69). White painstakingly demonstrates that even though Madison was deeply concerned with controlling the effects of factions, one should not jump to the false conclusion that Madison believed all individuals, behaving as individuals, could not be trusted to act justly: "Madison disparaged the motives of individuals acting in *political groups*. I emphasize the phrase 'political groups' because I do not think that Madison held that the hearts or inward parts of all individual men are depraved, corrupt, and filled with evil motives" (p. 95). Rather than holding a bleak or pessimistic picture of human nature, White characterizes Publius's humanity as a darker shade of Humean gray: individuals are neither angels nor beasts.

In this otherwise excellent book that offers a sophisticated presentation of the philosophy contained in *The Federalist*, there are a few philosophic questions left unexamined. White fails to question the implications of treating the three authors of *The Federalist* as if they were one. Given the eventual rupture between Hamilton and Madison, a discussion of treating Publius as a single, philosophic persona seems warranted. It is also questionable to assume that *The Federalist* presents a complete, or even the best, understanding of the philosophy contained in the Constitution; an examination of the debates at the Constitutional convention and at the state ratifying conventions would be essential to accomplish that monumental task. White also fails adequately to justify his position that Madison did not believe that all political groups are inevitably factious. White's position is contrary to that held by David Epstein (whom White draws upon at other moments) through his careful reading of *The Federalist* (*The Political Theory of "The Federalist,"* 1984, pp. 64–65). Finally, with the notable exception of a powerful discussion of slavery, White does not *critically* assess the philosophy of *The Federalist*. Perhaps this should be the final step in this influential scholar's illuminating explication of the United States' founding; that is, a *critical*, philosophic evaluation of the values and dreams of the founders. Although historians may want more historical evidence to substantiate some of his claims and political scientists will want a fuller appreciation of the political realities behind much of Publius's thinking, all serious students of the early republic will find White's book another valuable, clearly written, illuminating contribution to understanding our past.

RICHARD K. MATTHEWS

Lehigh University

Capital Punishment and the American Agenda. By Franklin E. Zimring and Gordon Hawkins (New York: Cambridge University Press, 1987. xviii, 192p. $22.95).

Although those who support the abolition of the death penalty in the United States will find little to cheer about in the recent *McCleskey* decision of the Supreme Court, they may find some comfort in this combination of careful

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scholarship and passionate argument produced by Zimring and Hawkins. The book focuses our attention on issues that social scientists raise and the legal profession has too often ignored. What factors were associated with abolition in European countries? What political changes might lead to its ultimate disappearance in this country? The role of public opinion in countries where abolition has been accomplished, the relationship of de facto and de jure abolition, and the off-again-on-again pattern in many European jurisdictions—these are all factors that might have enhanced the debate in the *McCleskey* case and that the authors use to build a case that the United States "will be catching up with itself" when it abolishes capital punishment (p. 165).

Some of their points are not new but are forcefully made: "It is no coincidence that the list of actively executing countries matches that of politically repressive countries" (p. 6), a fact that Amnesty International has noted in its current campaign for abolition. One of the most interesting sections examines the questions raised, especially by Justice Blackmun, in the *Furman* opinion and notes how the other justices failed to respond directly and forcefully in a way that might have shaped future Court decisions (pp. 50–73).

Their summary of the evidence against deterrence is well done but, as they note, probably beside the point as far as the debate itself is concerned. If, however, the death penalty does indeed serve primarily as symbol and its supporters are guided more by "faith" than "empirical evidence" or "ethical argument" (p. 19), then two conclusions follow. The first is that reasoned debate such as this book might spark will be useless. The second is that a change in political leadership is a necessary precondition for a policy change.

In predicting the future they are cautious for the short run and seem to shy away from direct political predictions. Certainly, they note that the role of the Supreme Court is both complicated and critical (p. 102). Yet what many feel would be the strongest predictor of change is mentioned only once, on page 157, when they note, "Developments in abeyance for the duration of the Reagan Court might quickly have come to pass during a Mondale administration." In sum, who is in the White House decides who is on the Court that decides the future of capital punishment. Especially if, as they suggest, elites will have to go ahead of public opinion on this issue, who wins national elections may well be the critical variable.

This book ought to be read by every thoughtful advocate of change in our system of punishment. It not only makes a strong (if not unique) case for abolition of the death penalty, but it also indicates many of the political factors that have been at work to continue death sentences. For example, understanding that states' rights rather than law and order may well be the key to interpreting post-*Furman* response (p. 44) can sharpen the focus needed to bring about real change. As a resident of the state that executed Velma Barfield (pp. 103–4) while political leadership allowed savage public behavior to direct public policy, I hope this volume receives the attention it deserves.

MARY THORNBERRY

Davidson College

COMPARATIVE POLITICS

Military Power and Politics in Black Africa. Edited by Simon Baynham (New York: St. Martin's, 1986. 333p. $32.50).

In the words of the editor, this book "aims to provide a comprehensive overview of military power in black Africa, from the period since independence but with considerable emphasis on quite recent and contemporary events." The book accomplishes its aim. While some of the contributions are uneven, the totality of the scholarship is valuable in understanding the broad dimensions and substance of the role of the military in black Africa. Equally important, the contributors represent a wide range of experience

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in African matters and most have a wide reputation as scholars of the first order.

The book is divided into three parts: an introduction, a part on the internal influence of the military, and a part on the international dimensions of military force in black Africa. The scope of the subject makes the book susceptible to criticism for its selection of topics and balance. Much of this criticism seems to be anticipated by Baynham's excellent and perceptive introduction. Not only does he highlight and summarize each contribution, but he provides his own valuable interpretation and assessment of the subject matter.

In the first part, Samuel Decalo, Claude E. Welch, David Goldsworthy, and Annette Seegers provide excellent chapters in examining the internal influence of the military. These range from Decalo's examination of the behavioral motivations of soldiers who become involved in political action and Claude Welch's perceptive study of military disengagement from politics to David Goldsworthy's examination of various views regarding the impact of military coups, military rule, and the idea of the "inevitability of instability" and Annette Seegers' insights into a much ignored subject, the evolution and character of armies of revolutionary states, specifically Mozambique and Zimbabwe. Aside from their own focus, the general thrust of the contributions conclude on a pessimistic note regarding the likelihood of continuing military rule, even when such rule is not a great deal different from civilian rule.

The second part of the book begins with another valuable contribution by Dennis Austin on the use of force to protect and perpetuate colonial boundaries in Africa. This is followed by contributions by Arnold Hughes and Roy May, Anthony Clayton, Abbott A. Brayton, and J. E. Spence. Hughes and May study the use of African armies as instruments of foreign policy. In general support of their views, Clayton shows the impact of the many foreign military interventions in African states. Brayton probes these matters in the context of arms control and concludes that among African states there is military restraint with effective arms control—a possibility only by the cooperation of major external powers. Finally, Spence analyzes the military in South Africa, arguing that its short-term interventionist policy in neighboring states has proven successful, but that such a policy is not likely to succeed in the future.

Contrary to the standard menu for reviewers of edited volumes, it is the view here that contributions should be assessed on two levels: their relevance to the purpose of the book and whether each contribution can stand on its own. Although one may disagree with some of the conclusions and assessments of individual contributors, the fact is that each has provided a scholarly study in its own right. Yet at another level, the book would have been more balanced had more attention been given to the anti-Marxist revolutionary military that has emerged in Angola, Mozambique, and Ethiopia, for example. Additionally, the study of external power involvement seems somewhat superficial in its treatment of the United States. Further, while some attention was given to why there are no coups in certain states, a much more comprehensive treatment of that subject is needed in a volume on the military in Africa. Finally, although scholarly objectivity requires some degree of circumspection, it would have been challenging to readers if more of the contributors had speculated on future developments, for instance, in South Africa. These minor criticisms aside, this collection is a valuable contribution to the literature.

SAM C. SARKESIAN

Loyola University of Chicago

Interpreting the Third World: Politics, Economics, and Social Issues. By Jacqueline A. Braveboy-Wagner (New York: Praeger, 1986. ix, 357p. $39.95).

To the question she poses, "Is there a Third World?" the author responds, "definitely yes" (p. 347), and in this text for undergraduates she examines in 10 chapters and a short conclusion the major characteristics of the Third World through three lenses: colonialism, modernization, and development. The result is a survey that covers much familiar ground but is somewhat unusual in the emphasis accorded different topics.

If there is a Third World, the question is how to get students from the First World, with all their readily identifiable cultural traits and biases, to engage in serious, sustained study of

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it. In both geographical and topical terms, as the subtitle of the work indicates, the Third World is a vast and varied subject. Within the literature on the Third World are examples of a number of approaches, each of which attempts to persuade students to shed their ethnocentric blinders and observe closely the other settlements in our global village. One approach, represented by Paul Harrison in his *Inside the Third World*, attempts to involve students emotionally and intellectually with frequent references to the conditions of ordinary people living in Third World countries, in circumstances very different from the students' own. A quite different approach is used by Braveboy-Wagner, who concentrates on the big picture, complete with many tables of national data of representative Third World states, with only the briefest references to the lives of ordinary people. These national data are loosely evaluated within the context of current major theories in an effort to introduce students to the theorists and their writings in the field.

According to Braveboy-Wagner, the Third World consists of those states that in general share the characteristics of having been colonies of Western powers, are technologically underdeveloped, are resistant to Western values, and are unwilling to align themselves with one of the superpowers. Having defined the Third World states, the author opens the first major section of her study with a discussion of colonialism and its aftermath. In contrast with most studies, which first examine the impact of colonial rule on the social, political, and economic structures of Third World societies, the author emphasizes the "international dimensions of nationalism" and the attempts by newly independent Third World states to create international cooperative movements such as the pan-Arab, pan-Islamic, and pan-African ones, on the grounds that "the Third World has institutionalized its cooperation in such a way that fundamental changes and agreements can be achieved only by dealing with the whole" (p. 348), a conclusion which many may find premature. Examining another aspect of the political aftermath of colonialism, that of nationalist conflict, the emphasis is again on its international dimensions, such as irredentist disputes and superpower interventions, rather than on the domestic economic, social, and cultural consequences of colonialism.

In the second half of the book, the distinctive but often related issues of *modernization* and *development* are considered, the discussion being shaped by general references to theorists like Parsons and Weber and their critics. Again, the stress is on the international aspects of development, such as international aid, rather than on the domestic, underscoring the author's commitment to discuss the "difficulties facing these countries and . . . the responsibility the international community has in helping the Third World resolve them" (p. 349).

Students using this text will have much useful data to consider, but they will not encounter illustrative cases of the consequences of hunger or illiteracy on specific individuals, or of the throbbing, jostling life of the slums. Interpreting the Third World is accomplished in this text through aggregate data, and beginning students are unlikely to experience much of an emotional response to Third World conditions. They will, however, have some useful data to ponder and will have been introduced to some of the insights of theorists with which to begin their own interpretation of the Third World.

HARVEY G. KEBSCHULL

North Carolina State University

Israel: Building a New Society. By Daniel J. Elazar (Bloomington: Indiana University Press, 1986. xi, 287p. $29.95).

In the prolific output of Daniel J. Elazar certain themes have often been present: federalism and intergovernmental relations, local government, covenants as the basis of law and politics, the concept of political culture as a methodological tool, the mixture of traditional and new elements in emerging societies, and a devotion to constitutional principles. In his new book, some of which is familiar from earlier essays, Elazar uses these themes in his rigorous, sophisticated examination of the factors relevant to the creation and development of Israel. But though Israel can be usefully studied within the context of comparative politics as an example of a newly emergent political system, it is also, as Elazar suggests, unique because of its link to the Jewish people.

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Early theories of modernization seemed to suggest that an increasing separation of church and state would accompany development. Empirical evidence from many countries has belied such a simple relationship. Israel provides a particular example of this by its increasing concern about its Jewish authenticity and the search by part of its population to link the state to appropriate forms of Jewish religious expression. In his own concern to link contemporary and ancient Israel, Elazar propounds a somewhat strained parallel between groups—the Saducees, Pharisees, and Essenes in Biblical times, and their contemporary approximations: secular Zionism and a civil religion, the Orthodox who value Jewish religious norms, and the communal kibbutz.

Though sometimes repetitive, Elazar, in this thoughtful and engaging study, paints a distinctively original portrait of the nature of Israeli society. He constantly presents his empirical analysis in the context of an elegant and often challenging theoretical structure. He also implicitly argues a case—based on a number of his favorite themes—that might be termed intellectual Sephardism at its best.

Throughout his book Elazar stresses the significance of federal principles in both Jewish affairs—the World Zionist Organization and early Zionist societies—and in Israeli society, from the covenants of the colonies in the first Aliyah at the end of the nineteenth century through the kibbutzim and the linkage of moshavim for cooperative purposes to the accommodation of the various groups to reach a working relationship. The three major Zionist camps—Labor, civil and religious—existing in a triangular relationship to each other, have led to what Elazar calls "a compound republic" rather than an organic state. Israel has been built by highly autonomous elements that surrendered a minimum of their power to a central authority. Paralleling this is the tension between *mamlachiut* (strengthening of state institutions), and *halutziut* (the pioneering spirit or voluntary cooperation).

Amplifying the familiar terminology of Almond and Verba, Elazar discusses the emergence of an Israeli political culture out of the three existing cultures; the subject culture of many Jews, with government seen as malevolent and limited concept of political participation; the statist culture of modernizers who see the state as an instrument for social change; and the traditional Jewish culture, civic and republican, in which God is sovereign and rulers are bound both by the Torah and by popular consent.

Contemporary Israel is an amalgam of all three strands; the first now represented largely in Arab villages whose inhabitants are instructed how to vote by clan leaders, the second shown in the bureaucracy, and the third—seeing power as federal and the result of a series of covenants—is reflected in the Israel Defense Force. But it is not always clear from Elazar's analysis how the emergent political culture will embody both the sense of national unity required by both security and Jewish isolation and a common vocation of building Israel, in which the federal or contractual element resulting from the amalgam of kinship and consent must be an important part. His view that the Israeli system has shifted from ideological democracy (based on doctrinal faithfulness) and sectoral groupings to territorial democracy is not wholly borne out by the complex mixture of traditionalism and civil society, and the unresolved tension between the concept of Israel as a Jewish state and the view that it is to be a light unto the nations.

Perhaps the most interesting—because least familiar—pages of the book are those on local government and politics. Local government has been important not only in providing and administering governmental services but also as a base for recruitment of political leadership, for fostering channels of communication between rulers and ruled, and for maintaining diversity in a small country. Elazar sees local affairs as the likely direction for future political life. The number of settlements enjoying municipal status has increased as have both the number of regional councils and the Arab and Druze villages with modern municipal governments. If the new state, between 1948 and the early 1960s, absorbed functions once in the hands of local, voluntary, or party bodies, it is now experiencing a trend to decentralization, partly because of the great growth in the number and size of cities, partly because of the pattern of more independent voting in local elections, and partly because of the emergence of a local leadership, often from groups otherwise largely excluded from politics.

It is on this last factor, the unduly neglected significance of the Sephardim, that Elazar is most distinctive. He tries to dispel the myth of

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two Jewish cultures and argues, at times in polemical fashion, that the Ashkenazim did not have a monopoly of Zionist pioneering and that the Sephardic cultural heritage is consonant with modernization. He denies that the Sephardim are more aggressive, more anti-democratic, and more hard-line on occupied Arab territory than the Ashkenazim. Differences in the levels of modernization of the two groups he sees as largely the consequence of inequality of opportunity in education, in occupation, and in economic matters like receipt of German reparations. By the late 1950s Sephardim obtained leading positions in local affairs, in the development of towns and rural settlements. On the national level, they moved to the Likud, which, unlike the Labor camp, welcomed them. Elazar extrapolates from this useful and novel analysis a controversial conclusion that the Likud is likely to become and remain the majority party in the country. Notwithstanding one's views on this particular issue, all students of Israeli politics will be grateful for and benefit from the acuteness of analysis, the penetrating shrewdness, and the commanding sweep of this book.

MICHAEL CURTIS

Rutgers University

The Making of a Liberal Democracy: Senegal's Passive Revolution, 1975–1985. By Robert Fatton, Jr. (Boulder: Lynne Rienner, 1987. ix, 189p. $25.00).

Senegal is a country plagued by grave and deepening poverty, mired in economic stagnation, with a government in thrall to international creditors holding it to a policy of severe austerity. It is also a functioning liberal democracy with a dozen active opposition parties. The leader of the strongest one in recent years has rivaled the president in popularity and some of the others advocate revolutionary socialist policies and sharply criticize the neo-colonial subsurvience and capitalist direction of the current government. How can liberal democratic practices be established in such a setting? Could other poor and economically troubled countries achieve similar democratic openings? What difference does democracy make, given the failure of the rulers of independent Senegal to prevent the deepening impoverishment of the majority of the people?

Robert Fatton, Jr. turns to Gramsci and Poulantzas for the concepts with which to address these important questions. Some of his answers will be surprising to those who accept the widespread view that poverty, traditionalism, peasant economies of affection, IMF adjustment measures, entrenched class cleavages, patron-clientism, and statist measures to develop industry and agriculture all militate against democratic practices. Fatton holds "not only that liberal democracy is possible in underdeveloped societies, but that it might present the best vehicle for the persistence of ruling class power" (p. 18). His analysis is stimulating and interesting for its theoretical imagination as well as for its perspective on Senegal.

In the late 1960s—with peasants blaming the government for diverting peanut earnings to feed the bureaucracy while the producers were going hungry, labor unions chaffing at the declining real value of the minimum wage, Senegalese business leaders pushing for a better deal in relation to French capital, and university students and other intellectuals outraged by the continuing power of the French presence—the country entered into a period of organic crisis. Fatton here makes excellent use of the existing studies of Senegalese society and politics. The ruling class, he shows, lost its capacity to impose leadership, while the lower classes were too divided and disorganized to mount an effective political challenge. But the rulers were strong enough and sufficiently skillful to elaborate and to carry through a passive revolution: *passive* because "although authoritarianism was replaced by the politics of hegemony, the structures of power remained fundamentally unchanged" (p. 53).

One key to the transition (and here Fatton brings a new variable into the equation) was the work done by a set of organic intellectuals, the "managers of legitimation" (p. 64) whose job it was to convince the lower classes that the politics of hierarchy was in the general interest. Another was the political skill of presidents Senghor and Diouf and the political elite of the country. A third was the largely complementary hegemonic power of the Muslim marabouts over the peasantry. The fourth element was the persistence of patron-client politics as a form of "coercive dependence" (p. 102) limiting class conflict in a class-divided

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society. Fifth was the divided and inchoate nature of the challenge posed by the revolutionary Left. And finally, the passive revolution was underwritten by the French military presence ready to defend the existing class alignments. This combination may make Senegal an exceptional case rather than a model for other peripheral societies; but Fatton seems uncertain about this, perhaps because his method eschews measures of the strength of social forces and stresses the existence of dialectical alternatives.

There are other ambiguities. It is not entirely clear what groups belong to the "ruling class." Nor are the reach and impact of the writings of the "organic intellectuals" plumbed. Are they constructing a powerful hegemonic ideology, or might they simply be currying favor with those in power? Popular political discourse in Senegal at all social levels takes place in Wolof, not French. It is a spicy mix of personal and policy, economic, and symbolic matters. How this popular discourse might relate to hegemony receives no direct attention in this book.

The analysis permits contradictory conclusions and Fatton does not shrink from drawing them. The passive revolution establishing liberal democracy "embodies . . . the death of fundamental change" (p. 165), but it also "represents a still fragile foundation on which more profound democratic values and practices may be erected" (p. 170). It "has curbed the intensity of opposition . . . and blurred the horizon of options" (p. 166), but "it has also unfolded a dynamic of reforms that may generate among subaltern classes a momentum for more radical demands" (p. 169).

Despite some imprecisions, ambiguities, and gaps, both the form and the content of Fatton's analysis are helpful and stimulating both for the study of Senegal and for the study of democracy. It has broken a trail others are sure to follow.

JONATHAN BARKER

University of Toronto

State and Countryside: Development Policy and Agrarian Politics in Latin America. By Merilee S. Grindle (Baltimore: Johns Hopkins University Press, 1986. ix, 255p. $25.00 cloth; $11.50 paper).

The Transformation of Mexican Agriculture: International Structure and the Politics of Rural Change. By Steven E. Sanderson (Princeton: Princeton University Press, 1986. vii, 324p. $42.00 cloth; $10.95 paper).

These books are significant both for their substantive findings about trends in agriculture in Latin America and for the theoretical issues they address. The authors generally agree on substantive trends, but they diverge interestingly with respect to how to characterize the state and interpret its role in agrarian politics. Sanderson's approach ("new international division of labor") tends to depict the state as a homogeneous actor with a single motivating logic and to relegate the state to a marginal role, essentially in facilitating international market forces. Grindle assigns greater importance to the politics and complex rationales that influence state action in agrarian policy, but she reaches similar conclusions about the consequences of state behavior.

With respect to substance, the books demonstrate how agriculture has played a subordinate role in import-substituting industrialization in the Latin American region since the early 1950s. In brief, population growth, urbanization, rising incomes, and international trade patterns have reinforced market forces in the countryside such that cattle, sorghum, oilseeds, and other tradeable goods have benefited, to the relative neglect of food staples (e.g., black beans, rice, maize) traditionally consumed by lower-income and marginalized groups. This has implied greater competition for scarce, arable land and increasing misery of the rural poor. The thrust of government policy has facilitated resource flows to commercial agriculture. At the same time, governments have sponsored reform programs first to distribute land, then to develop selected regions, and finally to integrate the rural poor in more comprehensive programs to raise productivity and living standards. These reform efforts have delivered limited benefits to the poor, but have proven useful in complex state strategies of cooptation and repression.

Sanderson's book begins with a theoretical

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chapter that reviews current approaches to the study of development and adopts one that implies "a domination by trade relations and by the transnational integration of production itself, not in the context of empire, but through the medium of the less nation-bound internationalization of productive capital" (pp. 19–20). This is followed by chapters on the more significant traded commodities (fresh fruit and vegetables; the frontier beef industry; and basic grains) and a discussion of the shifting logic of state intervention in Mexican agriculture. The strengths of the discussion are clearly in the treatment of fruit and vegetables and beef. Here Sanderson's impressive mastery of agronomy, economics, and trade patterns and his familiarity with the U.S.-Mexican border economy are brought to bear to support his thesis that market forces tend to overcome whatever ameliorative projects the state might undertake.

Though still solid, Sanderson's treatment of basic-staples agriculture and the complex government apparatus involved in production and distribution seems less surefooted. And his discussion of the Mexican Food System (SAM) suffers from a neglect of the complexity of the politics of government policy-making and bureaucratic infighting. This is due less to lack of knowledge (given the recency of the program) than to the depiction of the state as a unitary actor and an "argument [that] follows a more parsimonious concept allowing the state to follow the economic logic of the age: rational self-interest" (p. 234). Nevertheless, Sanderson's conclusion constitutes an important hypothesis: "The separation of production increases from agrarian reform leads to a highly undesirable effect: the state's modes of intervention in the agricultural economy . . . have been shown to *enhance* the internationalization of the Mexican agricultural system, not to challenge it" (pp. 280–81).

Grindle's book casts a broader comparative net to include specific case studies of Mexico, Colombia, and Brazil, with a thorough grounding in the pertinent literature on other Latin American countries. She sets out to challenge the important contribution by Alain de Janvry, *The Agrarian Question and Reformism in Latin America* (Baltimore: Johns Hopkins University Press, 1981)—which stresses the logic of capitalist accumulation in the periphery—because of her belief that "state elites have a variable capacity for autonomous decision making and often have specific interests in national development that cause the state to become active in shaping economic and social relationships with dominant-class interests in society" (pp. 3–4). Grindle also has a distinctive view of *reformist policies*, referring "only to efforts by the state to address problems of rural poverty, exploitation, and inequality. General policies for agricultural modernization are thus not considered reformist" (p. 195).

Grindle rejects both neo-Marxist and dependency approaches in favor of the Weberian tradition and a view of the state that "as an executive and administrative apparatus for decision making and control, has identifiable and concrete concerns about the definition and pursuit of 'national development.' These concerns are independent of, but not necessarily opposed to or different from, the immediate interests or welfare of any particular group, class, class fraction, coalition, or alliance in society" (p. 17). This perspective allows her to focus on political factors, such as ideology, leadership, bureaucratic complexity, and policy implementation. Further, the state is assumed to be neither autonomous nor homogeneous: "The relative autonomy of the state is assumed to be a desirable condition sought after by state elites insofar as it presents them with greater flexibility" (p. 18).

The discussion of theory is followed by chapters on agrarian history, the state and the ideology of modernization, patterns of commercialization of agriculture, survival strategies of the rural poor, agrarian-reform ideology and politics, and the "New Dualism" of integrated rural-development programs. Her conclusion pulls together her arguments that agrarian-reform projects are products of ideological commitments conceived in complex state bureaucracies and that the programs have been significantly reshaped by interest politics in the course of implementation. Her data and findings essentially reinforce those of Sanderson. But her approach to the state leads to different expectations. Grindle finds, for example, that "even state intervention often entrapped smallholders in market relations that caused a loss of security and control over productive decisions" (p. 125). Sanderson would have introduced the same remark with *of course*.

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Both authors make significant contributions, which makes the theoretical dialogue even more impelling. Sanderson would guide us to the international division of labor, concluding that the state is an insignificant actor, one limited to working out the details of exploitation: "In a word, the globalization of the Mexican food system has left the national state without competent mechanisms to govern rural growth and development" (p. 275). Grindle would agree that state intervention has largely advantaged the wealthier, commercialized farm interests; but her approach holds out the possibility of greater state independence and the possibility of more complex and nuanced government behavior.

JOHN BAILEY

Georgetown University

Elections in Independent Africa. Edited by Fred M. Hayward (Boulder: Westview, 1987. xvii, 318p. $23.85).

This is a volume in the Westview series, "Special Studies on Africa," composed of an introduction and conclusion by the editor, and eight country studies. The cases examined are Sierra Leone, Ghana, Nigeria, Botswana, Tanzania, Zaire, Kenya, and Senegal. Like the editor, many of the authors have been affiliated with the African studies program at the University of Wisconsin.

This is clearly one of the most important and innovative works on African politics in recent years. It is important because of its continent-wide perspective on a recently neglected aspect of African political processes. More significant, however, is the potential contribution it makes to our understanding of the function of elections generally. The case needs to be made more often that Africa is a rich laboratory in which to study the variety of human political experience; this book can contribute powerfully to reducing parochialism in comparative politics.

Area specialists often find it difficult to write about their region for a general audience. The specialist often unwittingly limits the impact of a case study by making assumptions about the audience's prior knowledge of local context. These authors have not entirely succeeded in overcoming this problem, and it is unfortunate that the studies with the best summary descriptions of political history are those on Nigeria and Zaire, already the best known to a general audience. Still, the chapter introductions are generally sufficient to make the analyses comprehensible, and they carry the reader to a rich testing-field of major propositions on electoral politics advanced in Dahl and Huntington's classics.

These descriptions document the presence in most African traditions of the expectation that people will have some measure of control over political authority. Although the electoral process was itself new to those traditions, it was easily accepted in the context of decolonization. With independence, many leaders saw elections to be threats to their power and took steps to eliminate them as effective measures of participation; yet there remains "a powerful desire for mass participation in Africa" (p. 281).

Hayward notes that in only one of the cases examined (Sierra Leone) has Africa seen an election result in a change of government. The centrality of elections in African politics clearly diminished after independence. Furthermore, the sample of countries examined here is clearly biased toward the minority of countries in which the electoral tradition is strongest. Still, there is powerful evidence in this work of the need in many countries for electoral legitimation and of the degree to which leaders are pushed to make the electoral process credible. It is unwarranted to dismiss elections as irrelevant in the African context when we unthinkingly accept the validity of the process in American and British history. The pervasiveness of the patron-client relationship in these African countries would bear comparative examination with nineteenth century voting patterns in the advanced Anglo-Saxon models. The one-party dominant regimes in Botswana, Senegal, and Kenya could be closely compared not only to that of Mexico but to Japan and its Liberal-Democratic Party. Mass-elite linkages are invigorated by elections in strikingly similar fashion in all these cases.

Hayward imbeds in his conclusion the assertion that "African experience with elections over the past decades has much to suggest that enriches our understanding of the political process" (p. 280). This theme should be given more prominence, for *Elections in Independent*

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Africa has much to offer in developing our knowledge of the evolution of electoral systems and in fitting that knowledge to major theoretical writing on the subject.

ROBERT J. MUNDT

University of North Carolina, Charlotte

The Kuna Gathering: Contemporary Village Politics in Panama. By James Howe. Latin American Monographs, no. 67 (Austin: University of Texas Press, 1986. xvi, 326p. $27.50).

The 30 to 35 thousand San Blas Kuna inhabit numerous small near-shore coral islands along Panama's northeastern Caribbean coast. To the anthropologist, the Kuna are a fascinating people for a variety of reasons: they are relatively recent migrants to their island environment (and thus yield important data for various studies of adaptation), they are one of the few twentieth century indigenous success stories, maintaining a remarkable degree of autonomy within the confines of a nation-state (aided by a successful revolution—with a little help—in 1925), and they are an intensely political people. The Kuna have been the focus of numerous ethnographic studies, particularly since the late 1960s.

James Howe has been conducting research among the Kuna since 1970. His interests in the Kuna (reflected in his publications) extend well beyond political topics to include kinship, ritual, mythology, cosmology, oral literature, subsistence, ethnosemantics, and ethnohistory. In his own view, one amply supported by the content of the book under review, "almost all [of these topics] have political implications and have contributed in important ways to my understanding of the social and cultural context of Kuna politics" (p. 264). In *The Kuna Gathering*, he provides us with the first full-length account of Kuna local village politics. His account is lucid, readable, and theoretically informed. Howe avoids rigid adherence to any particular theoretical viewpoint, but this is not to say that the study is atheoretical. Rather, it highlights the remarkable extent to which general models may require revision in the face of empirical data. Each standard theoretical lens through which Kuna politics is viewed leaves it anywhere from slightly to considerably out of focus.

His methods of data gathering include many of those deemed standard in anthropological research: participant observation, structured and unstructured interviews, listening to and recording conversations, paying informants to keep diaries, taking censuses, copying documents, and the administration of a standard comprehensive questionnaire—in this case on political organization—to informants from as many villages as possible. (Details of his field research are presented in an appendix.)

Most of the book is set in the decade from 1970 to 1980, but enough historical information is presented (in an introductory chapter) to assist understanding of how things got to be the way they are, with considerable attention given to significant changes and to the adaptability of the system. Sufficient contextual description is provided throughout the book to permit discrimination between general features of Kuna political behavior at the village level and their contextually dependent variants.

The core event of Kuna politics is the *gathering*, an almost nightly affair in many villages. The gathering has two modes: the sacred, singing gathering and the secular, talking gathering. Both play important—and distinctive—roles in politics and public life. The traditional core hierarchy of elective offices in both forms of gathering are those of chief, *arkar* (spokesman or interpreter), and policeman. Today in many villages political offices have proliferated but all officeholders must report to the talking gathering and abide by the collective decisions made there. Howe focuses in this study on the talking gathering but does devote two chapters to the singing gathering, one a description of a typical singing gathering and the other an analysis of the significance of the sacred gathering complex.

The singing gathering provides at the village level "the social and symbolic foundation for more directly political action" (p. 78). At its core is Father's Way, the collection of all the Ways or Paths (e.g., stories of the spirit world, cosmology, history, morality tales, and contemporary events and narratives), which the chiefs chant and the *arkars* interpret, filled as they are with metaphor, while policemen exhort the audience to stay awake. Howe argues that Father's Way is, among other things, a political constitution in metaphors.

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Eight chapters are devoted to various facets of the secular, talking gathering, the scene of direct political action. At this secular gathering, the political aspects of matters of concern to the village are discussed and decisions made.

After outlining the characteristics of Herbert Lewis's tripartite typology of leadership in prestate and early state societies—*free enterprise, kin-based,* and *republican*—Howe demonstates in two chapters, one focused on chieftainship and the other more generally on public office, that the Kuna political system closely approximates Lewis's republican type. Attention is also given to F. G. Bailey's conceptualization of *support* as essentially a dyadic relationship between leader and follower. Howe shows that this does not fit the Kuna case, where "support consists more of a decision about a leader, and less of personal allegiance to him. . . , [where] each voter decides between candidates in terms of a personal calculus [rather than] a model of personal followership" (p. 102). Several other features of public office are also examined in detail.

One chapter is devoted to a presentation of a representative evening at a talking gathering. The focus is on speech events and the variety of functions that they serve, principal among them being *informing* (expected of a chief and regularly done), *admonishing* (regarded as a primary task of leaders, especially of chiefs), and *adjudicating* of trouble cases.

A chapter devoted to public works is concerned principally with communal labor. Howe is considerably more optimistic regarding its future in Kuna villages than is Holloman, another Kuna specialist. Both agree that there is considerable stress on the system. Disagreement over communal labor tasks has caused village fissioning in at least four recorded cases. Holloman assumes that the system functioned more smoothly in the past, an assumption contradicted by Howe's ethnohistorical data. Howe proposes "that disagreement, resentment, heated discussion, and even occasional schismatic conflict be considered normal parts of village task organization. . . . the inevitable miscommunication, mistakes, unfairness, and breakdowns in communal labor inspire considerable dissatisfaction among the Kuna. Neither this unhappiness nor the Kuna propensity to pessimistic hand wringing should blind us to their overall success at getting things done" (p. 149). Howe's position is supported by the fact that close to 20 years have passed since Holloman's research and there has been no noticeable collapse of the system of communal labor. In fact, the system seems remarkable in its capacity to incorporate additional tasks.

Four chapters examine—among other topics—the distribution of influence (Howe prefers this term to *power*) and patterns of alignment; political action and process and the role of outsiders in village politics; the political aspects of dispute and trouble-case adjudication; and conflict and village fission. A final chapter discusses three problematic theoretical issues in the light of Kuna politics: influence and leadership, political culture and contradiction, and democracy. Throughout the book, Howe's analyses are careful and stick close to the facts of the matter, and his arguments are thoughtful and thought-provoking. Given space limitations, I shall comment on patterns of alignment and process only.

Howe takes the position that the standard concepts—interest group, following, faction—are of little utility in analyzing Kuna patterns of alignment. He notes that "a key principle of Kuna politics is that each man acts in the gathering house primarily as an individual" (p. 185) with the result that there is tremendous variation in alignments; that is, alignments are issue-specific. Nonetheless, general viewpoints or orientations held by individuals may result in more consistent alignments than would be predicted by a strictly issue-specific model. In the Kuna case, traditionalist and modernist orientations bear directly on issues involving innovative change, and individuals are more likely than not to be consistent in holding to these orientations. This may very well produce more regularity in patterns of individual alignment than would be apparent from an examination of issues one by one.

Howe portrays the dominant process models in political anthropology as characterized by conflict and competition, as a basically antagonistic game consisting of a series of intermittent episodes interspersed by periods during which the protagonists marshall their forces. He does not argue that such episodic conflict does not occur among the Kuna but rather that Kuna politics cannot be understood as a series of discrete episodes. Thus, while his overall orientation is one of process and there

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is attention to conflict and its consequences, Howe's Kuna model attends as much to harmony as to conflict, as much to continuity as to periodicity, and as much to outcomes as to the processes by which these are achieved. Howe states that standard process models neither comfortably fit, nor tell us very much about, Kuna politics. He contends that insufficient weight has been given in current process models to the "verbal nature of politics" (p. 196). He convincingly uses a single case to display the broad range of near universal kinds of argumentation employed by the Kuna in striving for consensus through pursuasive argument in their gatherings. That they often fall short of this ideal and achieve only grudging acquiescense or majority rule goes almost without saying. But as Howe correctly notes, it is the collective nature of Kuna decision making that deserves emphasis, not the fact that the process may not always result in consensus.

Overall, in *The Kuna Gathering*, Howe provides us with a vivid, largely qualitative, microview of Kuna village-level politics while at the same time judiciously attending to a broad range of issues of current concern in political anthropology and political science. The book is a valuable contribution to the study of politics in small-scale societies—and a pleasure to read.

PHILIP D. YOUNG

University of Oregon

Reagan, Thatcher, and the Politics of Decline. By Joel Krieger (New York: Oxford University Press, 1986. viii, 247p. $29.95).

This is an ambitious book about an important subject. The *decline* in Krieger's title really turns out to refer to several distinct, though related, developments: the declining competitiveness in world markets of both the British and U.S. economies, the decline of an international order marked by U.S. hegemony, the decline of a set of institutions and policies dubbed by Krieger the *Keynesian Welfare State*, and the decline of the political coalitions supporting that Keynesian Welfare State. These changes plainly affected all advanced capitalist nations. Krieger's concern is to show that they produced a distinctive political response in the United States and the United Kingdom, to trace the common elements of that response and to suggest how and why Reaganism and Thatcherism nevertheless differ in key respects.

This ambitious task is accomplished in a quite straightforward way. Krieger begins with pictures of the "Calvary" on which the Callaghan and the Carter administrations were crucified at the end of the 1970s—economic stagnation, labor unrest, and military failures. He sketches (chap. 2) the rise and fall of the Labour coalition that was so closely associated with the development of the Keynesian Welfare State and performs a similar analysis in tracing the decline of established ideologies of Conservatism and their displacement by Thatcherism. This is succeeded by a sketch of the changing ideological and structural setting of British politics in the 1970s, and of Thatcherism's exploitation of the resulting divisions within electorates. The focus of the U.S. chapters is more internationalist—on U.S. imperial decline, on the challenge of foreign economic competition, and on how structural change means that a "Sunbelt consensus has emerged to replace the old Keynesian consensus" (p. 143).

The book's main thesis is manifestly true—"that when the generalized economic crisis of the 1970s beset Western Europe and the United States, in the United Kingdom and the U.S. in particular, neo-conservatives and new right forces would be better positioned to take advantage of the political-electoral openings" (p. 22). What is distinctive is the racy and provocative way in which the thesis is elaborated.

First, Krieger draws an excessively clear distinction between the kinds of coalitions supporting the Keynesian Welfare State and those supporting Reaganism and Thatcherism. On the one hand, the Keynesian coalition is pictured as resting on a "historic vision of a society motivated by social justice" (p. 110); on the other, new coalitions are pictured as the creation of social division and mere political arithmetic. But this neglects the key point about the Keynesian Welfare State—that it, too, rested on alliances that were instrumental in character and that it, too, defended some sectional interests at the expense of others. Both the Keynesian Welfare State and its successors are based on the arithmetic of sectionalism.

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Second, Krieger seems to me grossly to overstate the magnitude of the change that has occurred, at least in the case of Britain. This is nowhere clearer than in his treatment of nonwhite immigrants and their descendants whom, strikingly, he persistently calls *black*. Thus Thatcherism is pictured as the culmination of the "gradual transformation of black citizens into trespassing aliens" (p. 102). To put it mildly, this glosses a complex history—including the highly varied experience of different kinds of New Commonwealth immigrants and the development of an established body of anti-race discrimination legislation.

This is a quarrel on a narrow point, true, but it is symptomatic of a more general difficulty with Krieger's account, which is that some of his key conceptual categories are less than rewarding. Thus much is made of "suburbanism" as a characterization of Thatcherism. But the Conservative party has been suburban for a century. That is part of the secret of its success. Nor are the distinctive elements of "suburbanism" very convincingly described. Thus, Krieger refers to "the xenophobia of the private housing estate" (p. 63) as one element; yet the burden of evidence is that xenophobia is, if anything, more common to proletarian life.

MICHAEL MORAN

University of Manchester

Comparative Democracy: Policymaking and Governing Coalitions in Europe and Israel. By Gregory M. Luebbert (New York: Columbia University Press, 1986. xiv, 341p. $32.00).

Despite its misleadingly general title, this is a thought-provoking book on a specialized topic: the formation of governing coalitions in multiparty democracies.

Luebbert begins by examining government coalition formation in a number of multiparty systems, develops a general theoretical formulation to account for the observed patterns, then returns to these countries to test, or flesh out, the theory. His general theory relates the patterns of coalition formation to properties of the democratic regime through an analysis of the goals and rational strategies of party leaders. He buttresses the theory with some broad statistical comparisons of 67 cases of government formations in Norway, Sweden, Denmark, Finland, Italy, the Netherlands, and Israel, as well as detailed applications to Israel, Denmark, and two periods of Finnish coalition formation. The empirical analysis draws on an impressive array of primary and secondary sources.

The starting point for Luebbert's analysis of the coalition formation itself is the party selected as the *formateur*, which has the responsibility for building the coalition. Other parties' policy goals are analyzed as convergent, divergent, or tangential (focused on different issues) in relation to those of the *formateur*.

Most parties are assumed to have a limited set of decisive policy principles—their "policy profiles"—that are relevant to coalition participation. The policy profile is usually derived from the history of the party and defines its identity to its supporters. On issues outside the profile, party leaders can negotiate relatively freely. But on the decisive party profile issues, the leaders can seldom negotiate without jeopardizing their leadership position.

According to Luebbert, different types of democratic systems differ in the incentives they offer to parties to cooperate, inside and outside of the government arena. He develops a fourfold typology of regimes—consensual, competitive, conflictual, and unconsolidated—based on the legitimacy of the regime and the role of opposition parties in policymaking. The incorporation of electoral and policy-making factors outside the narrow legislative arena is an important feature of the analysis. Within the competitive type, he further distinguishes between dominated and undominated systems, depending on the presence or absence of a party whose presence is believed to be essential for all future coalition governments. He then predicts the limits of policy tolerance that affect patterns of party cooperation in each type of system.

This analysis not only leads to specific predictions about government coalitions in each case, based on the party profile relations and their implications for cooperation in each type of system but to explanation of patterns of coalitions. We can thus understand the appearance of minimum winning coalitions in undominated competitive systems; the frequent appearance of minority governments in

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the consensual systems and, for different reasons, in the conflictual ones; the appearance of oversize government coalitions in the dominated competitive systems; and so forth. It is, of course, the ability to explain these latter events that makes Luebbert's theory innovative and important.

The work is highly suggestive but seems to contain notable limitations. At the theoretical level, I am troubled by failure to incorporate some of the factors usually considered in coalition analysis; for example, the loss of patronage and of policy implementation control necessitated by bringing additional parties into the coalition. The emphasis on policy profile goals to the exclusion of other considerations seems overstated. On the empirical side, the analysis of coalition formation in seven nations using four or five system configurations inevitably makes the results highly dependent on nation-specific patterns. Luebbert does include several interesting within-nation changes in system configuration. But the combination of an inferential approach and an analysis in which there are nearly as many relevant configurations as countries make the empirical results more suggestive than confirmatory, despite the accuracy of the retrospective predictions.

I have no doubt, however, that some of the innovative elements proposed here will become part of any mature theory of coalition formation. Luebbert's work departs from the large theoretical and empirical literature on this topic in several very interesting ways. The results are sufficiently impressive to merit serious attention from scholars working in this area.

G. BINGHAM POWELL, JR.

University of Rochester

Authoritarians and Democrats: Regime Transition in Latin America. Edited by James M. Malloy and Mitchell A. Seligson (Pittsburgh: University of Pittsburgh Press, 1987. 268p. $25.95 cloth; $12.95 paper).

The recent breakdown of most of the bureaucratic-authoritarian (BA) and "reactionary despotic" regimes in Latin America and the earlier demise of the corporative-authoritarian systems in Spain and Portugal have given rise to a growing "transition to democracy" literature, to which *Authoritarians and Democrats* makes an exceedingly useful contribution. Of particular value is the manner in which a number of the authors have interpreted the well-known 20-year cycle of dictatorship and democracy in the region.

Mitchell Seligson argues that although the permanence of the current democratic phase is by no means assured, just as the BA regimes of the 1960s and 1970s represented a "new breed" of authoritarianism, the current democratic regimes are also of a different order from those of the 1940s and 1950s. This is due to three factors flowing from the character of the bureaucratic-authoritarian regimes that preceded them: a much greater reluctance by civilian political leaders "to consider the 'military option' a convenient way out of mounting economic and/or political problems" (p. 9), the inability of the military establishments to manage their national economies effectively, and the fact that the contemporary democratic governments have come to power at a time in which the minimum levels of socioeconomic development necessary for democracy—defined as an income level of 250 per capita 1957 dollars and a literacy level of over 50%—had been reached in most of the countries.

Applying his thesis to Central America, Seligson found that although there was considerable lag time, as those nations approached the minimum threshold of socioeconomic development there was a tendency for them to create civilian democratic governments and to return to authoritarianism when they dropped below that threshold. The current paradox in the situation is that while the rapid economic growth from 1960 to 1975 pushed all of these nations, except Honduras, above the economic threshold for democracy, the resulting income inequality generated by that growth has raised the level of insurrectionary activity.

In his very interesting contribution, "The Peruvian Experience," Luis Abugattas argues that the traditional association of populism with democracy in Latin America and antipopulism with authoritarianism obscures the fact that they really constitute two distinct, although interrelated, cycles. Peru, he states, has experienced both democratic (1963–68) and authoritarian (1968–76) populism, classic authoritarianism (1976–80), and antipopulist

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democracy (1980–85)—all brought about by the classic growth-distribution dilemma.

James Malloy enhances our understanding of the double cycle by pointing out that while organized labor in Latin America tended to be populist, labor leaders often had more access to social benefits under authoritarian governments than they did in democracies. "In the end," he contends, "access to power and control over distributable goods is the name of the game, not forms of regimes" (p. 252).

This judgment appears to be largely confirmed by the other contributors to the volume. Aldo C. Vacs points out that at the very time President Bignone was beginning the redemocratization process in 1983, most of the major economic sectors in Argentina were demanding "the formulation and implementation of the state-led programs of reactivation and distribution better suited to their interests and goals" (p. 30). Similarly Silvia T. Borzutsky argues that it was primarily the failure of the monetarist experiment in Chile that almost destroyed the Pinochet regime in 1983. Finally, James Malloy and Eduardo Gamarra contend that the inability of General Hugo Banzer to manage the clientalistic demands in Bolivia's "neopatrimonal" society led to the move toward civilian rule there in 1978. Only in Brazil, Silvio Duncan Baretta and John Markoff argue, was the *abertura* largely driven by political considerations.

The essays in this book, including additional chapters on Ecuador, Central America and U.S.-Latin American relations, are of a uniformly high quality and make a valuable contribution to our understanding of Latin American politics over the past two decades.

ROLAND H. EBEL

Tulane University

The Politics of the West German Trade Unions: Strategies of Class and Interest Representation in Growth and Crisis. By Andrei S. Markovits (New York: Cambridge University Press, 1986. xx, 599p. $54.50).

The German Trade Union Federation (*Deutscher Gewerkschaftsbund* [DGB]) and its 17 constituent unions comprise the strongest and most effective union movement of any large industrial democracy, and Markovits has written the first book in English to show extensive knowledge of this organization. The first three chapters cover union structure, labor law, and the history of the federation. The history emphasizes DGB programmatic demands, conflicts between activist and moderate union wings, and relations with parties and governments from the 1949 birth of the Federal Republic to 1981.

Four subsequent chapters deal respectively with the activist metalworkers union (with over a third of DGB members the largest union in the noncommunist world); the moderate construction union; the chemical union riven itself by activist and moderate wings; and the oldest German union, the printers. The four are all private sector unions, and each chapter is heavily involved with union wage determination, collective bargaining, and strikes. Since each of these chapters and the DGB history are written chronologically, much repetition ensues. The issue of the 35-hour week, for example, appears in chapters on the DGB (pp. 146–53), the metal union (pp. 252–63), the printers union (pp. 410–14), and in the conclusion (pp. 432–37). The conclusion summarizes, updates from 1981 to 1984, and covers challenges to the unions in the crisis years since 1974, particularly rapid technological change and high unemployment.

There is an enormous literature by and about the unions, and Markovits has digested a sizable part of this and produced accurate and readable chapters on the DGB. The readability of the union chapters suffers somewhat in the details of one after another wage struggle.

Facts are generally handled well, but a problem with judgments seems to derive from overexposure to the more radical activist union point of view and very little exposure to empirical studies of West German politics. Unionists tend to an apocalyptic style of writing. On the side of the angels, they fight a sinister and powerful capital; and when union demands are not fully met, a great defeat has occurred.

Thus, the 1952 codetermination law, which gave labor a third of the directors on the boards of large firms, was a "disastrous setback" (p. 83). It did not match the equal seating of labor and capital on boards in the coal and steel industries given by the British in

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1947 and secured under West German law in 1951—"the unions' only success during the period under consideration and for some time to come" (p. 77). This judgment simply ignores the difference between *no* seats and *one-third*, and in addition all kinds of highly beneficial labor and social laws that were passed in the 1950s with strong inputs from a Christian Democratic labor wing, Social Democrats, and the unions.

Among many other questionable judgments are "The political 'independence' of the unions has undercut their ability to influence the policies of both individual parties and the federal government" (p. 28), "Capitalism in West Germany could not destroy labor (as happened in the Weimar Republic)" (p. 65), The "restraining qualities [of laws governing industrial relations] have on the whole outweighed the benefits for West German trade unions" (p. 419), and "Labor will by definition remain unsatisfied with the operation of government given West Germany's capitalist framework" (p. 425).

Two serious factual mistakes appear in the chapter on the construction union as a part of a diatribe (uncharacteristic of other parts of the book) against a former president—a leading moderate, Georg Leber. Leber's winning a seat in the Bundestag in 1957 supposedly "made him the first major union official to hold a seat in the legislature since Weimar" (p. 346). But prior Bundestags included three national union presidents (Freitag, Imig, Jahn), two presidents of DGB state districts, and 11 members of union and DGB national executive boards. Also, "Actual earnings in construction remained lower than in all other major industries in West Germany" under Leber (p. 357). Federal statistical yearbooks, however, report that construction workers were sixth highest out of 24 industries in 1955 and tied for seventh with iron and steel workers out of 36 industries in 1966, the years spanning Leber's stewardship of the union. This chapter, as well as the one on the printers, was "written with Thomas C. Ertman" (p. 327). Still, overall responsibility must remain with the senior author.

RICHARD J. WILLEY

Vassar College

Government and Private Sector in Contemporary Mexico. Edited by Sylvia Maxfield and Ricardo Anzaldua Montoya (San Diego: Center for U.S.-Mexican Relations, University of California, 1987. iv, 146p. $3.00 paper).

For much of this century, private business in Mexico has survived in a postrevolution constitutional system in which it does not share equal political status with other groups within the ruling Institutional Revolutionary Party (PRI). By the mid-1980s, however, increasing numbers of business people and organizations have become more militant, advocating a new political party for business, supporting the major opposition party, the National Action Party (PAN), or increasing civic education about business. This is occurring in a period of considerable change within the international and the domestic economies. The search by business for a satisfactory place in the Mexican social pact is explored in rich detail in this collection, detailing conflicts between business and the state and within business itself, as both the state and business attempt to adjust to the internationalization of the political economy.

In the introduction, Sylvia Maxfield offers a clear overview of the search by private business for an appropriate political role and of its divergent approaches to gaining more influence within the system. She notes—but might have expanded upon—the growing cleavages between the professional economic technocrats in the government and those who shape policy within the PRI. These conflicts could be critical to future economic policy.

The analysis by Matilde Luna, Ricardo Tirado, and Francisco Valdes traces the growth of increased political militancy by business since the nationalization of banks in 1982. Attacks on the presidential system, development of truly competitive political parties, and creation of a free market economy are included among the demands of militant business voices.

The free market is precisely what won't work, contends Ricardo Carrillo Arronte, who believes it is "outdated" because private business is unable to raise production to necessary levels for Mexico. Sharply critical of the export of capital by Mexican entrepreneurs, he argues that business is not willing to

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reestablish confidence in the present system.

Apart from conflict between business and the state, sharp differences among Mexican business people and organizations constrain the exercise of political influence by business, according to Saul Escobar Toledo. Tracing political differences among business organizations between 1976 and 1986, he, nonetheless, believes that present conditions may be right for the exercise of increased political influence by business.

A closer look at one of the major business organizations—COPARMEX—the Confederacion Patronal de la Republica Mexicana is provided by Luis Felipe Bravo Mena. Examining five phases of the organization's relations with the government, he regards the last five years as especially conflictual. While COPARMEX, as an employers union, does not aspire to political power, it "plays the political game, but it does so outside the party arena."

Celso Garrido Noguera and Enrique Quintana Lopez point to the structural change underway in Mexico in the 1980s from what had been the old "hegemonic bloc" controlling the distribution of capital and resources to a new, still-fragile, reconstituted bloc. The new "bloc" includes international banks, former Mexican bankers, reorganized industrial interests, and new elements in the state economic bureaucracy. These may be joined by other new sources of foreign exchange—drug traffickers and *maquila*-manufacturing enterprises.

In any case, the way Mexican businesspeople perceive themselves and their social-political roles is likely to shape their future political behavior, according to the study by Roderic Aix Camp. The attitudes and images of Mexican businesspeople are in flux, but Camp believes that more are accepting the concept of social responsibility and are defining themselves politically in a more assured and self-confident manner and more are inclined to play a political role.

Although according to most authors structural transition may be occurring in Mexico, none is convinced that Mexican business will achieve greater political influence in its relations with the state and within the society. Indeed, increased militancy could further fragment society and lead to powerful reactions by the state and by other social actors.

This is a most useful and detailed account and analysis of the political role of private business in Mexico in the 1980s, for the ultimate definition of this role will determine much about Mexico as it moves into the twenty-first century.

JAMES W. DULL

University of New Haven

Party and Participation in British Elections. By Anthony Mughan (New York: St. Martin's, 1986. vi, 159p. $29.95).

Anthony Mughan's new book is innovative and thought-provoking. In the past decade political scientists interested in the study of voting and elections have made increasing use of time-series aggregate data. Much of the resulting research has focused on the impact of macroeconomic conditions on support for governing parties and their leaders. In *Party and Participation in British Elections* Mughan expands the scope of this emergent research tradition by using aggregate data gathered over a 33-year period (1950–83) to investigate factors influencing electoral turnout.

Mughan's decision to focus on turnout rather than vote shares is a welcome departure in the face of the voluminous literature on the latter topic and the paucity of studies of the former one. The lack of attention to the "turnout dimension" is particularly apparent when one considers the corpus of work on non-U.S. political milieux. Mughan is acutely aware of this lacuna in the literature, one that he explains as follows: "It is an undeveloped branch of the study of electoral behaviour precisely because the contribution of short-term forces to the explanation of turnout . . . has been largely ignored" (p. 6).

Short-term forces are, of course, many and various. Mughan's particular focus is on the efforts parties make to bring voters to the polls. He argues that such political mobilization efforts, while not completely neglected in previous research, have not been adequately studied. In *Party and Participation*, Mughan considers the impact of mobilization on three (dependent) variables: (1) constituency-level turnout in the several general elections occurring between 1950 and 1983; (2) turnout in 240 by-elections taking place in this period; and (3) the difference in turnout in particular constitu-

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encies in successive general and by-elections. Based on a host of detailed analyses Mughan argues forcefully that party mobilization is an important determinant of turnout in each of the three cases.

The argument is intuitively appealing and accords well with other recent research on the significance of short-term forces in models of electoral behavior. However, the analyses upon which it rests are likely to be viewed as problematic by some readers because of the way Mughan operationalizes his key independent variable, that is, mobilization. His constituency-level aggregate data do not provide direct measures of parties' mobilization efforts. To circumvent this problem Mughan relies on interparty competition as a proxy variable. Competitiveness at the constituency level "can be taken as a surrogate measure of the organization mobilization of support undertaken by political parties especially, but also by organizations affiliated to them and other partisan and non-partisan groups and individuals" (p. 50). Although it seems likely that levels of party competition and parties' mobilization efforts are positively related, the proposition that the latter is adequately indexed by the former is an empirical question—one that the author does not answer.

Other aspects of the analyses are provocative as well. One example is the neglect of constituency-level socioeconomic and demographic characteristics as factors that might influence turnout (and interparty competition and party mobilization as well). Mughan justifies ignoring these "nonpolitical" variables by arguing that "the explicit focus of this analysis is the politics of turnout and to include in it non-political variables would only risk obfuscating this focus unnecessarily" (p. 12). Granted Mughan's additional point that many such variables change only slowly, their neglect may prove troubling for readers concerned with the possibility of model misspecification caused by the omission of theoretically relevant variables. A second, related, example is the failure to include a measure of the strength of voters' partisan predispositions. Several studies have documented the weakening of partisanship in the British electorate in the post-1970 period, and it is plausible that this trend has had consequences for various forms of political participation including turnout. More generally, the failure to consider possible time-series dependencies in the data created by the dynamics of relevant long-term forces might be judged by some as a risky research strategy.

Overall, most of the contentious aspects of the analyses presented in *Party and Participation* arise not because Mughan is unaware of important theoretical and methodological points at issue but rather because he is the captive of a data set that is not fully adequate to the research task at hand. It is abundantly evident that he is well aware of the limitations of his data—indeed, he repeatedly draws them to the reader's attention. Moreover, cognizance of these limitations prompts speculation about a variety of interesting questions that might be pursued in subsequent inquiries. In the end, Mughan's failure to address some of these questions within the framework of his empirical analyses is less important than the contribution he has made by raising them. Those doing subsequent research on electoral participation in Britain or other Western democracies will benefit by carefully considering the research agenda established by this thoughtful study.

HAROLD D. CLARKE

Virginia Polytechnic Institute and State University

Political Parties and Elections in West Germany: The Search for a New Stability. By Stephen Padgett and Tony Burkett (New York: St. Martin's, 1986. xii, 308p. $32.50 cloth; $14.95 paper).

West German Politics in the Mid-Eighties: Crisis and Continuity. Edited by H. G. Peter Wallach and George K. Romoser (New York: Praeger, 1985. viii, 278p. $34.95).

From 1969 through 1982, when two worldwide recessions punctured the myth of boundless prosperity, when Western nations floundered in their transition toward the "postindustrial" age and were pronounced "ungovernable," one country in the Atlantic Alliance managed to keep the same party(s) in power: West Germany. True, even there change finally caught up with the government, but not until everyone else had gone first. In fact, throughout the whole postwar period,

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West Germany had fewer chancellors than the United States had presidents or Britain prime ministers.

Never mind. Accounts of German politics de rigueur carry apocalyptic overtones. As if "continuity" was suspect and "crisis" more natural; as if the country had not found its political bearings but was still on a "search for a new stability." Even Wallach and Romoser as well as Padgett and Burkett feel compelled to cast their contributions in a mold of uncertainty, though only in their subtitles and introductions. Once immersed in the stream of politics, neither of them, nor the various contributors to the Wallach and Romoser volume, find much that is unsettling in West German politics. In fact, they all reassure us of the pragmatic, adaptive nature of politics in West Germany. What is surprising is how unsurprising West German politics is, how skilled at "crisis management," how deft in negotiating a *Machtwechsel* (change in power).

West Germany in the Mid-Eighties, edited by Wallach and Romoser, the latter a co-founder of the Conference Group on German Politics, focuses on the "critical election" of 1983. Chancellor Schmidt's coalition government of the Social Democratic Party (SPD) and the Free Democratic Party (FDP) had collapsed in September of 1982 and had been swiftly replaced by a new coalition composed of the Christian Democratic Union/Christian Social Union (CDU/CSU) and FDP. The March 1983 election was called to give the new parties in power the popular license to operate the government. The actions of the political parties leading up to that election are reviewed, with considerable historical depth, by Patterson (on the CDU/CSU), Braunthal (SPD), Soe (FDP) and Buerklin (Greens). The economic context is provided by Wallach, and the international setting by Hanhardt. Dalton and Baker dissect the electoral verdict of 1983.

Soe's contribution on the FDP, the "pivot" party, is a gem, a riveting story of a party flirting with self-destruction but without whose political high-wire acts the Federal Republic would not be governable. Soe chronicles the metamorphoses of the FDP from "residual bourgeois party" in the early 1950s through its social-liberal phase in the late 1960s back to economic laissez-faire, with Count Lambsdorff rising as a reincarnated "Erhard of the 1980s." We learn in suspenseful detail how the FDP reached its decision in 1982 to quit Schmidt's government and forge a new coalition with the CDU/CSU under Helmut Kohl, with the names of the FDP deputies in favor of the move and opposed.

The electoral analysis by Dalton and Baker raises some intriguing points. For all the claims of an ideological *Wende* (U-turn) and the sound and fury of the "missile" campaign, the ordinary German going to the polls apparently did not endorse a sweeping policy cleaning—either a mandate for the deployment of new intermediate missiles or a dismantling of the welfare state. In fact, on a battery of key issues, the "CDU/CSU is most proximate to the electorate on not a single issue. Even the Greens are seen as the most representative party on three issues" (p. 52). Yet, as the authors also demonstrate, the public rated the CDU-led government as better able than an SPD-led government to handle almost every issue that mattered. So much for (position) issue voting in West Germany!

Political Parties and Elections in West Germany, by Padgett and Burkett, who teach politics at Loughborough University, England, for the most part chronicles party history over the past hundred years. There is no pretense of this being the definitive work on the history of German parties. Nor is this an analysis to settle scholarly controversies about party development. A mildly leftish undercurrent streaks through the flow of events now and then.

Padgett and Burkett emphasize the split personality of the Social Democratic party, beginning with the rivalries of its patron saints, Ferdinand Lassalle and Karl Marx. But their own account also displays a split personality. The pre-1933 SPD gets a scolding for lack of revolutionary zeal: "The successful and savage destruction of the German Revolution [of 1918–19] was accomplished by the surviving forces of Imperial Germany, encouraged by the quivering comrades [SPD] who now ruled the state" (p. 38). On the other hand, the post-1945 SPD is upbraided for its "refusal to join a coalition with the 'bourgeois' parties" (p. 46).

The freshest chapter of their book is the one dealing with Germany's newest party, the Greens. The authors stress that this is "by no means a single issue party. It is rather, a party of protest against modern industrial society and its economic, political and social effects"

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(pp. 181–82). The Greens are the partisan vanguard of "post-materialism," the harness of the "new social movements" of the 1970s. How the Greens as a party will do in the future depends largely, the authors argue, on how this party will answer the *Gretchenfrage* of a coalition with the SPD. Their chapter on the electorate, on the other hand, reads like a stale recitation of demographic patterns. The *Forschungsgruppe Wahlen*, whose data are cited here, polls on many more fascinating items than social class and religion. We learn little about reasons for the ups and downs of party fortunes in elections.

On the whole, however, the Padgett and Burkett book offers undergraduate students a handy compendium on party development. The Wallach and Romoser volume, in turn, fills the role of a *Germany at the Polls* for 1983, with crisp accounts of each party's background. My advice to the two editors: reissue the volume with updates on the 1987 election and with a more affordable price tag.

HELMUT NORPOTH

State University of New York, Stony Brook

The Impact of Illness on World Leaders. By Bert Edward Park (Philadelphia: University of Pennsylvania Press, 1986. xxiii, 377p. $24.95).

Political scientists have long been interested in the study of leaders, and such work has typically focused on, for example, childhood socialization, wealth, education, social class, and psychological characteristics. For the most part, political and other social scientists have ignored the fact that leadership, at least at the head-of-state level, is normally a phenomenon of the middle to advanced years. As such, the diseases often associated with aging are relevant to an understanding of the behavior of leaders. The burden of this excellent volume by Bert E. Park, a practicing neurosurgeon with graduate training in history, is that a precise knowledge of medical science coupled with standard individual-level historical and political analyses will yield powerful information about the real behavior of political leaders, information that goes far beyond what we have come to expect from the discipline of political science.

The political behavior and medical histories of eight leaders are carefully examined. Woodrow Wilson, Franklin D. Roosevelt, and Adolph Hitler have been selected for extended treatment, while briefer case studies are developed for Paul von Hindenburg, Ramsay MacDonald, Jozef Pilsudski, Winston Churchill, and Anthony Eden. "Each leader," Park tells us, "was considered among the greatest movers and shakers of his day, yet all were afflicted by neurologic illness that impacted negatively on their leadership skills and decision-making abilities" (p. xix).

Methodologically, the author undertakes a remote medical diagnosis of each leader-patient by using memoirs and other primary source materials, combined with the most recent medical knowledge. To those potential critics of ex post facto medical analysis, Park asserts that "as 80 percent of diagnosis in medical practice relies on the medical history, and only 20 percent on physical examination, the latter is relatively unimportant in such medico-historical analysis" (p. 79).

The results of this methodology are impressive, and two examples can be offered. The academic controversy regarding whether Franklin Roosevelt was or was not competent during his last years in office is carefully addressed by Park in his examination of the president's hypertensive encephalopathy, which is a transient disorder causing extremely high blood pressure during which the patient is confused and disoriented. After an attack passes, the patient returns to a more stable and unconfused posture. Thus, Roosevelt experienced alternating psychomedical states, and this explains why historians and other analysts have had difficulty in interpreting his observed political behavior.

Most examinations of Adolph Hitler have been anchored in psychoanalytic theory, but Park points out that a broad range of organic factors had important behavioral influences. Hitler, physically ill for an extended period of time, was afflicted by Parkinson's disease, chronic cholecystitis (inflammation of the gall bladder), depression, temporal lobe epilepsy, and polypharmacy. The latter is especially important: Hitler's physician was treating him with over 70 medications including overdoses of two known poisons, atropine and strychnine. Moreover, there is strong evidence that Hitler regularly used cocaine and synthetic

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morphine derivatives. As Park carefully illustrates, these illnesses were important determinants of Hitler's political behavior.

One of the rewarding strengths of Park's analyses is his sensitivity to the behavioral correlates of disease and medications. This, combined with his skillful analysis of decision-making processes, nicely integrates the multiple dimensions of political behavior within a single individual. Moreover, so effective is this work, that it speaks strongly to reorienting much of what we call *political decision making* to the individual level of analysis.

I do have some problems with the book. For a volume packed with a considerable amount of scientific detail, it would have been helpful to the reader to have the text broken up into subsections for ease of reference and for pulling out comparative data. Beyond the chapter titles, no subsections exist.

I found the concluding chapter disappointing, not for what it said but for what it did not say. This chapter deals essentially with the provisions of the 25th Amendment to the U.S. Constitution. However, since only two of the eight cases in the volume were U.S. citizens, the applicability to other political systems is limited indeed. A cross-national focus on disability would have been useful.

Park is strongly critical of the physicians who treated the leaders under study here. A considerable amount of professional incompetence and deceptive behavior is documented. An extended reflection on this would have been helpful. Why did heads of state not recruit the best medical talent of the day as their private physicians? How should physicians for heads of state be chosen? How should they be removed when found incompetent? Why did the political entourage around each head of state tolerate medical incompetence and deception? The author provides the beginnings of responses to some of these questions throughout the book, but the answers never come together in a coherent way in the concluding chapter.

But these criticisms are minor. Park has produced a volume that is destined to become a classic interdisciplinary study of individual-level political decision making. It deserves serious attention.

THOMAS C. WIEGELE

Northern Illinois University

Political Authority in a Sicilian Village. By Filippo Sabetti (New Brunswick, NJ: Rutgers University Press, 1984. xi, 293p. $35.00).

Over the past decade-and-a-half a number of books have taken on the problem of the Mafia in Sicily, confronting the apparent paradox of an outlaw regime coexisting with a modern western state. From Anton Blok's *The Mafia of a Sicilian Village* (1974) and Jane and Peter Schneider's *Culture and Political Economy in Western Sicily* (1976) to Pino Arlacchi's *La mafia imprenditrice* (1983), they have asked whether there is any special cultural basis for the Mafia and how the Mafia has been produced by national and international political economic forces.

Sabetti, reconstructing the political history of a western Sicilian village over the past two centuries, tackles these same problems and is at pains to show that the Mafia arose not due to any Sicilian defects of culture or personality nor to any aversion to taking collective political action but is instead attributable to an administrative structure and national policies that robbed the people of their local autonomy. In this view, the Mafia was created as a means through which peasants could defend themselves from the caprices of extralocal powers, especially as these developed in the wake of Italian Unification in the 1860s.

One of the book's merits is the meticulous reconstruction of the relationship between the development of a Mafia system of power in this village and the development of Church-inspired and socialist social movements around the turn of the century. According to Sabetti, the Mafia grew up as a system of local consensus, sanctified by the local clergy. Hence, Sabetti argues against his predecessors' view that the Mafia developed as the exploitative arm of estate managers to maintain rural order and put down peasant attempts to assert greater power.

Sabetti also discusses the transformation of the local government in the post-World War II years "into a sort of trading company" of the Mafia (p. 168). Government patronage was dispensed to supporters and hurdles were erected to prevent opponents from availing themselves of the public benefits to which they were legally entitled. Yet according to Sabetti the Mafia collapsed shortly after this, in the

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1950s, following its transformation from a system that represented the interests of the peasants to one that was wholly parasitical. It was this loss of moral authority together with the increased strength of the national party organizations at the local level that brought about its demise.

Here we have a strikingly positive view of the development and functioning of the Mafia at the local level in Sicily. The Mafia is seen as a continuation of a local self-help tradition that lasted as long as it benefitted the villagers and then disappeared when it had outlived its usefulness to them. Rather than reflecting amoral familism, the political history of the village shows, according to Sabetti, a value system prizing "universalistic values of liberal democracy" (p. 235).

This is a surprising book, taking a thesis dear to Sicilian apologists—that all Sicily's problems derive from the national bureaucracy—and arguing that until the nineteenth century institutional arrangements in baronial Sicily were more conducive to local self-government than in more recent times. While provocative, it is unlikely to convince many scholars to overturn their less rosy view of the development and role of the outlaw regime, both because of questions of evidence regarding the relationship between the landowners and the Mafia at certain crucial points, and because, assuming Sabetti's portrait is accurate, the evidence from other parts of western Sicily make his village appear unusual. Blaming "general social disintegration" (p. 234) in Sicily on "institutional arrangements" and especially on centralization of power and lack of local autonomy, can be convincing only if it is shown that these institutional arrangements are different in Sicily than in northern Italy, which has a very different political culture. This Sabetti does not do, at least for the postwar years. Finally, it would also be helpful if more consideration were given to the movement of the Mafia from the countryside to the big cities and the change in the nature of the Mafia from an agrarian phenomenon to one principally involved in state parasitism in the form of living off development and construction contracts and the like, as well as the new-found wealth available through the drug trade. The decline of the Mafia in a small rural village of Sicily signals not the end of the Mafia but merely its moving on to greater opportunities elsewhere, a statement with which Sabetti would likely agree.

DAVID I. KERTZER

Bowdoin College

Crisis in the French Labour Movement: A Grassroots Perspective. By W. Rand Smith (New York: St. Martin's, 1987. xiii, 272p. $29.95).

W. Rand Smith asserts that most literature on French trade unions focuses on the pronouncements of the peak confederations rather than on the actions of union locals at the shop-floor and plant levels. He addresses the problem by studying union locals of two major trade union confederations, the Confederation Générale du Travail (CGT) and the Confederation Française Démocratique du Travail (CFDT), in four metalworking plants in Grenoble.

The introductory section describes the theoretical and historical setting of the study. Smith seeks to determine whether the crisis of French trade unions derives from economic factors, such as shifts in the occupational structure from manual to technical positions; from political factors, particularly splits within the French Left; or from institutional changes, especially unions' greater legal rights, which paradoxically may weaken them by separating leaders from rank-and-file workers. (Smith later emphasizes the importance of management and local union strategy.)

Part 2 describes the four plants that served as research sites, with particular attention to the CGT and CFDT locals, and reports findings from Smith's interviews with union militants. The background and motivation of rival union militants are remarkably similar. In particular, and contrary to expectation, CFDT militants are only slightly more religiously observant than their CGT counterparts. The most interesting finding is that militants report that their greatest problem—mentioned more than twice as often as their relations with management—is the distrust in which militants are held by rank-and-file workers! Unfortunately, Smith barely explores the possible basis for this distrust, which would seem central in explaining union difficulties.

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Part 3, which analyzes unions' activity during strikes, relations between CGT and CFDT locals, and union-management relations, is the richest section of the book. Smith finds that unions neither fully direct strikes nor merely capitalize on worker discontent. The unions' importance derives from their activities during periods of quiescence, when they raise workers' consciousness by offering alternatives to management's views. Smith asserts—cogently but without evidence—that this lays the groundwork for subsequent mobilization at a point when arbitrary management actions precipitate worker protest.

Smith's observations of CGT-CFDT relations at the plant level supplement accounts of the two unions' stormy relations at the national level. He suggests that CGT and CFDT locals tend to conflict during periods of quiescence but are pressured by workers to cooperate during strikes. However, when a settlement is reached, conflict recurs as each union seeks credit for gains that have been achieved.

Smith describes an important recent shift in management's strategy from confrontation to consultation, common elsewhere but quite surprising in France. He suggests, as Michael Piore (whom Smith does not cite) did several years ago that French management's initiatives represent a response to union advances in the post-1968 period. As unions became more institutionalized following the May 1968 uprising, management devised consultative techniques that bypass and thereby weaken unions. Smith's assertion is based on speculation. Interviews with management of the four plants would have been useful here. The book concludes with an assessment of the Auroux industrial relations reforms enacted in 1982, and concurs with other recent research that minimizes the importance of the legislation.

Smith occasionally overreaches, as when he claims that because previous research has studied unions at higher levels, it has focused "inordinately on union ideologies rather than union practices, on what unions say rather than what they do" (p. 7). Given the fact that industrywide and peak-level collective bargaining in France has regulated key aspects of work life, this assertion is quite misleading. Even more questionable, in light of the CFDT's strategic reorientation since 1978, is to consider the union anticapitalist (p. 179). And he remains faithful to the common practice of neglecting Force Ouvrière, the third major French trade union confederation, whose substantial gains are in marked contrast with CGT and CFDT decline.

Crisis in the French Labour Movement provides a useful addition to the burgeoning literature on French trade unions. Smith reminds us that national shifts reflect complex and often contradictory local developments. He underlines the importance of management and union strategy in shaping working class structure and activity, rather than accepting economic or technological determinism. (For example, he notes—without analyzing sufficiently—that technicians, engineers, and even lower-level management are unionized in the plants he studied.) Both because of what it accomplishes and fails to accomplish, *Crisis in the French Labour Movement* suggests the value of local studies of unions.

MARK KESSELMAN

Columbia University

The Primordial Challenge—Ethnicity in the Contemporary World. Edited by John F. Stack, Jr. (Westport, CT: Greenwood, 1986. x, 231p. $35.00).

The Primordial Challenge—Ethnicity in the Contemporary World, edited by John F. Stack, Jr., is a collection of 10 essays. The first 3 essays are primarily conceptual, outlining several approaches to the study of ethnicity. The remaining 7 essays are descriptive case studies of ethnic relations in particular countries.

The use of the word *primordial* in the volume's title and in the title of the editor's introductory chapter, "Ethnic Mobilization in World Politics: The Primordial Perspective," is misleading in both instances. None of the chapters argues for a primordialist conception of ethnicity; namely, that ethnicity is a thing sui generis, arising out of antiquity, prior in its origins, primal in its influence. Both Stack's introduction and his volume reflect a diversity of views and approaches to ethnic phenomena. This diversity opens the door to some interesting essays. Stack fails to provide an overarching framework, however, and the result is a very loosely constructed book.

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An example of an essay somewhat adrift is chapter 2, "Theories of Ethnic Relations," by Michael Hechter. Reflecting on the state of ethnic relations theory construction, Hechter identifies three necessary dimensions of any complete theory of ethnic relations. Such a theory must account for variations in (1) ethnic group cohesion, (2) rates of ethnic mobilization, and (3) relative strength of ethnic group formation (compared to, say, class). Hechter provides a cursory discussion of the literature and finds little evidence of successful theory construction. The fact that the essay was written in 1978 (a fact noted twice by Hechter) renders its conclusions essentially useless and raises questions about the author's and the editor's decision to include it in the volume.

Cynthia H. Enloe's chapter, "Ethnicity, the State, and the New International Order," though also somewhat dated, provides the reader with a much more current and lively exposition. Enloe argues that the significance and operation of ethnicity in modern states lie in the relationship among the state, ethnic groups, and the international system. The international system affects ethnic-state relations in two ways: (1) through the internationalization of the division of labor and (2) through the internationalization of state security. In this chapter Enloe discusses in detail only the first (though she has written extensively on the second). She catalogs several ways in which states coopt, coerce, exploit, and generally manage ethnic labor and discusses the ways in which the international economy influences state-ethnic relations. Enloe draws examples from a wide variety of states and her illustrations are lucid and convincing.

In chapter 4, "Visions of the Melting Pot in the American City: European and Native Expectations in the United States and Argentina in the Period of Mass Immigration," Mark D. Szuchman sets out to compare the immigration experiences of the United States and Argentina to "illustrate the results of . . . [elite] xenophilism" (p. 43) in Argentina. Szuchman begins by outlining the evolution of sociological and historical thinking about urban immigration among U.S. and Argentine scholars. He then presents two "overview[s] of immigrants" in U.S. and Argentine cities. These overviews reflect the disappointing weakness of this chapter. A promising agenda results in no real information on immigration patterns (When? Where? How many? Who?). One is left quite ignorant of the timing, origins, destinations, and experiences of U.S. and, particularly, Argentine immigrants. The chapter's main contribution is a brief but interesting discussion of the Argentine state's efforts to employ immigrants for rural development.

The next two chapters focus on interethnic relations in Miami. Chapter 5, "Immigration and the Politics of Ethnicity and Class in Metropolitan Miami," by Christopher L. Warren and John F. Stack, Jr., examines the ethnic political economy of greater Miami. Warren and Stack briefly sketch the patterns of immigration leading to the current triethnic (black, Latin, non-Hispanic white) composition of the Miami area, as well as the economic and social characteristics of each of the three groups. They describe the structure of the urban government that oversees an uneasy triethnic constituency. The authors point to two factors contributing to Miami's interethnic tensions: (1) economic competition among the three groups for jobs and control of the economic sector, as well as electoral competition for control of the city government and (2) the city government's inability to mediate interethnic disputes and ameliorate interethnic competition.

As its title suggests, chapter 6, "Ethnic Bargaining and the Noncitizen: Cubans and Haitians in Miami," by Anthony P. Maingot, focuses the social science lens more closely on two components of multiethnic Miami, Cubans and Haitians. Maingot presents the first systematic quantitative evidence in the volume when he traces the historical demographic changes in the numbers and percentages of blacks, whites, and Latins in Dade County from 1950 to 1985 (p. 88). The chapter goes on to describe the timing and reception of the major waves of Cuban and Haitian immigrants. Maingot depicts the early Cuban experience as one of political and economic incorporation, whereas the Haitian quest for citizenship rights has been one of struggle, organization, and mobilization. Maingot attributes the different experiences of the two groups as mainly the result of the economic and political context of the period and the class composition of the different immigrant groups, noting the difficulties faced by the later "Mariel" Cuban immigrants.

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Chapters 7–10 focus on ethnicity outside the United States. Steven L. Lamy examines the consociationalism in Belgium (chap. 7), Ralph S. Clem discusses patterns of regional economic development and ethnicity in the Soviet Union (chap. 8), Cheryl A. Rubenberg describes ethnic elitism in Israel (chap. 9), and Rene Lemarchand surveys interethnic conflict in sub-Saharan Africa (chap. 10). Unfortunately there is little in these chapters that is new or illuminating.

Lamy's chapter, "Policy Responses to Ethnonationalism: Consociational Engineering in Belgium," outlines the mainly European consociational recipe for multiethnic governance and describes the modification of Belgian consociationalism in the face of an insurgent Flemish constituency. The response of the Belgian state to Flemish nationalism was the adoption of ethnically proportional rules for public resource and job allocation, a reorganization of voting and governance, and an expansion of ethnoregional autonomy. Lamy concludes that these consociational changes equalized opportunity between Flemish and Walloon communities.

Clem's chapter, "Regional Economic Development and the Territorial Basis of Ethnicity: The Case of the Soviet Union," attempts to analyze differences in Soviet economic development among ethnoterritories in light of official Soviet policy of equality among the nationalities. He compares rates of urbanization among Russian and non-Russian territories at three points in time: 1926, 1959, and 1970. Clem finds that the period from 1926 to 1959 was characterized by a higher rate of urbanization among Russian territories than among non-Russian territories, but that the trend was reversed during the 1959–70 period. The result was reduced, but inequalities between Russian and non-Russian territories persisted. While Clem makes an effort to present systematic evidence, this chapter is flawed in two ways. First, it shares a problem with several other chapters: its results are dated. Second, and more important, Clem's use of urbanization as an indicator of economic development is highly questionable, especially in light of the vast literature on urbanization, particularly in the Third World, where urbanization is depicted as an index of poverty and agricultural decline, rather than a measure of progress.

Rubenberg's chapter, "Ethnicity, Elitism, and the State of Israel," argues that Israelis of Ashkenazi descent represent an ethnic elite. This is hardly news to anyone familiar with the literature on ethnicity or Israel. The chapter's section dealing with the relationship between ethnicity and Israeli international security on the one hand and Israeli political policies on the other is of some interest, as is Rubenberg's passing comment on the role of labor markets in mitigating ethnic tensions. Unfortunately neither discussion is elaborated.

In the last chapter, Lemarchand's "Ethnic Violence in Tropical Africa," the author, like Hechter, admits in a footnote that the chapter is dated. Unlike Hechter, however, Lemarchand does not disown his earlier views. Although the chapter is interesting, it is simply a reprise of now familiar themes; that African ethnic conflict is rooted in colonialism and in the construction of new multiethnic states and that African postindependence politics are the politics of ethnicity. Some themes are worth repeating, however, in particular that there is much to African ethnicity that is political rather than primordial. And some themes are usefully illustrated; for example, that "new" ethnic groups can result from the fragmentation, as well as from the fusion, of cultures. Lemarchand also usefully notes the role of "external variables" (p. 194), citing Angola as a prime example of East-West interventionism.

The Primordial Challenge addresses an interesting and important topic in the social sciences, but the volume lacks cohesion. Stack provides no useful conceptual or thematic unifying structure. Each chapter seems almost randomly placed. The chapters do not build on or complement one another. Stack's introduction wanders through a small portion of the ethnic relations theoretical literature and never refers to the subsequent chapters nor makes any effort to integrate them. He writes no chapter introductions nor does he provide a conclusion. Finally, since the majority of the articles were written in the late 1970s, the book was published several years too late. *The Primordial Challenge* fails to live up to the promise of its content and its title.

JOANE NAGEL

University of Kansas

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Marxist Local Governments in Western Europe and Japan. Edited by Bogdan Szajkowski (Boulder: Lynne Rienner, 1986. xvii, 216p. $25.00 cloth; $10.00 paper).

This anthology has as its purpose to elucidate the strategy and performance of Communist parties that participate in local government in Western Europe and Japan. Since all of the parties considered are nonruling Communist parties (CPs) operating within a democratic parliamentary framework, local government is seen as "the principal area in which they have been able to build up a substantial and consistent administrative record" (p. 1), and thereby establish their credentials as legitimate contenders for national power. As the authors of the introduction, David S. Bell and Bogdan Szajkowski, point out, while there have been many case studies on Western Communist parties, few have focused on the role of these parties in local government, and fewer still have provided a comparative perspective. This volume sets as its purpose the filling of this gap.

The book is only partially successful in fulfilling this purpose, however. The quality of the chapters is very uneven. This is perhaps due less to the capability of the various authors than to the great discrepancies in the intrinsic interest and relevance of the cases studied. Because of the electoral weakness of the CPs and their consequently marginal role in local government, some of the chapters—Britain, Greece, and Scandinavia (except Finland)—are very brief and consist almost entirely of an overview of the parties' electoral fortunes. Other chapters, although they deal with larger parties with important power bases in local government (Spain, Portugal, Japan), focus excessively on national strategies, electoral results, and electoral geography at the expense of a detailed analysis of the *performance* of those parties when they do participate in local government. The paucity of data on what these parties actually do at the local level can be explained by several factors: the centralized nature of the political systems in question, which allows little freedom of maneuver for local governments; the recent democratization of Spain, which has led all the political parties to focus their efforts on national-level politics; the closed nature of the Portuguese Party, which impedes empirical research into its activities; the fact that where these parties do participate in local government, they most often do so in coalition with other parties, especially the Socialists, thereby complicating the discovery of a distinctively *Communist* style of government or policies. Despite these qualifications, in a volume purporting to provide new insights into the behavior of Communist parties by directing attention to the local level, the lack of detailed consideration of the actual behavior of these parties in local government is a serious weakness.

The reader gets an effective feel for the performance of Communist parties in local government only in the chapters on Italy, France, and Finland. On the basis of these more systematic presentations together with scattered evidence from the other chapters, some generalizations about the role of Communists in local government and the role of local government in Communist parties can be drawn. In all the countries considered in this volume (with the possible exception of Portugal), the CPs have abandoned the Leninist strategy of violent revolution in favor of a parliamentary road to socialism. As a result, local government provides an important terrain where they can test concrete policies and, more importantly, acquire governing experience and an image of efficient and innovative administration that can be used to enhance their credibility as potential parties of government at the national level. This raises the fascinating question of the degree to which Communist behavior in local government is shaped by ideology or by a pragmatic adaptation to electoral needs and local situations. In those chapters in which the authors provide evidence as to the policies and priorities of Communist parties in local government, a pattern does emerge, despite the very real constraints imposed by limited powers and funds and the demands of coalition politics. Communists appear to promote grass-roots democracy and increased "openness" in local government, in terms of increased popular participation and greater accessibility of local government to the citizenry. This emphasis on citizen participation can take various forms—support for neighborhood associations (Spain) and even formal delegation of certain city government functions to elected neighborhood councils (Italy), as well as mass mobilization of the population on issues ranging from protests

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against central-government cutbacks in funds for social services to international issues like peace and nuclear weapons. Communists also tend to use their limited fiscal autonomy to promote redistributional policies in social-service provision, one of their top priorities being construction of public housing. Although such policies are far from revolutionary, and in many cases have been subsequently copied by non-Communist administrations, they do betray in their origins a distinctively leftist bias, demonstrating that ideology does make a difference, even though the local context places severe limits on the extent of that difference.

At the same time, Communist administrations in countries like Italy and France, where the fullest documentation is available and where Communists have administered a substantial portion of the country's urban population, have quite deliberately presented themselves not just as the voice of the working class, but as the guarantors of "clean" and efficient administration in the interests of the "whole population," thereby attempting to construct the broad cross-class alliances which would be necessary to carry them to positions of national power. This is especially important given the decline in their traditional constituency, the blue-collar working class. As a result, their policies and their governing style have been geared to compromise and consensus rather than to either class confrontation or direct confrontation with the central government. This is most clear in the so-called Emilian model in Italy and in the close working relationships between French mayors (including Communists) and prefects.

In virtually all the cases studied, Communist influence in local government appears to be on the decline. In the Conclusion, the editor attributes this decline to the persistence of democratic centralism as the organizing principle of CPs and the consequent subordination of local government to national policies and priorities. This is somewhat misleading, since in many of these countries *all* parties perceive local elections and local government as reflections of or testing grounds for national policies and priorities. Much more important has been the inability of left-wing administrators to come to terms with the increasingly serious problems of urban government (unemployment, social-service provision, violence) in a period of austerity, the limited powers of local governments to deal with these problems, and the lack of control by Communists over the policies and funding decisions of the central government. In addition, the left-wing alliances that gave Communists a role in local government even where they did not have a clear majority (France, Italy, Japan, Spain) have broken down under the pressures of political change at the national level. Finally, fundamental unanswered questions about their identity, strategy, and goals undercut the ability of Communist parties, despite their deep involvement in democratic politics, to construct essential social and political alliances and to present a convincing left-wing alternative.

JUDITH CHUBB

College of the Holy Cross

From Insurrection to Revolution in Mexico. Social Bases of Agrarian Violence, 1750–1940. By John Tutino (Princeton: Princeton University Press, 1986. xvii, 425p. $42.50).

For students of Mexican history, of agrarian history and peasant mobilization, and of social revolution, this is an important book. Original in its conception and masterfully executed, this comparative social history of Mexican regions over the course of almost two centuries addresses the fundamental question of why and when peasants rebel (and not so much for what). Offered as an "interpretive synthesis," the book does incorporate primary research by the author on north-central Mexican rural history. The bibliography attests to the author's thoroughness in covering the literature on the subject.

Intrigued by the contrast between 1810, when rural violence on the eve of Mexican independence from Spain did not lead to revolution, and 1910, when a revolutionary transformation of Mexico accompanied the outbreak of agrarian insurrection, Tutino examines not only developments surrounding these two specific periods but also the century in between, paying close attention to regional similarities and differences. He also considers the theories of Barrington Moore, Eric Wolf, and Theda Skocpol in searching for answers.

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Tutino contends that perpetual poverty, deprivation, and exploitation can only lead to the kind of outrage that expresses itself in violent reaction when certain social changes in the countryside cause these longstanding peasant grievances to become "acute," and when peasants perceive political opportunities to act without immediate repression. Such a moment occurs when a deterioration in the peasant's relative *autonomy* (ability to produce subsistence independently, for example, through ownership or access to land) declines without a compensatory rise in *security* (ability to attain subsistence consistently); concurrently, a divided elite weakens the state. In 1810, acute agrarian grievances were limited geographically, and the elite remained united in the face of peasant insurrection. In 1910, much more widespread social deterioration in the countryside was followed by political breakdown. A sustained and highly conscious peasant mobilization forced the postrevolutionary elite to incorporate anticapitalist agrarian reforms, thus preventing a full-blown capitalist development. (Whether the incorporation of agrarian demands weakens, or in fact, facilitates, capitalist development in Mexico can be debated but not within the scope of this brief review.)

Tutino's model results from his critique, modification, and subsequent synthesis of Moore and Wolf on the one hand, and Skocpol on the other. He faults Moore and Wolf for placing too much emphasis on capitalist incursions, thereby incorrectly identifying class conflict as the sole or primary force in social change. Skocpol's corrective goes too far in another direction, attributing too much importance to international causes for the breakdown of the state, which in turn allows for revolutionary insurrection. For Tutino, it is the *dependent* nature of Mexico's capitalist development—capitalism "forced" by an elite intent on closing the gap—that produces the conjuncture of acute grievances and state breakdown that makes revolution likely. In the end, however, Tutino affirms more than disputes Moore, Wolf, and Skocpol. His revision is tinkering and borrowing, not a major overhaul or comprehensive theoretical reconsideration. This book will be valued for some time as a significant contribution to our understanding of modern Mexico, and less for its impact on the general theoretical debate on peasant mobilization and social revolution. Perhaps that is the way it should be, for Tutino is first and foremost a very good historian.

EVELYN HU-DEHART

City University of New York

INTERNATIONAL RELATIONS

A Changing International Division of Labor. International Political Economy Yearbook Series, vol. 2. Edited by James A. Caporaso (Boulder: Lynne Rienner, 1987. xiii, 249p. $26.50).

The editor of this volume sets out three extremely broad goals for this handbook: (1) to identify the changing role of labor in the global production process; (2) to see what part labor is playing in organizing and shaping the new international division of labor; and (3) to analyze the major causes of structural evolution of the work forces in the advanced industrial countries and to see what relationship these factors have to comparable changes in less-developed countries.

This volume is of limited value as a handbook. It does not contain any systematic empirical presentation of the basic data that one would want before attempting generalizations in this field. Nowhere does it contain even rudimentary survey data on the size, composition, and income of national work forces. Nor is there sufficient comparative data to permit making judgments about how labor has fared under different types of economic-policy management. Although literally dozens of theoretical concepts are discussed or referred to, there is no common theme or methodology

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that integrates the chapters. It is thus reasonable to assess this volume as a collection of essays.

As individual essays, the chapters vary considerably in quality. The Higgott and Mytelka chapters are excellent and the Skak chapter is very insightful though a bit thin on quantitative justification of its principal claim. The other three chapter writers (Caporaso, Junne, and Jacobsen) are so mired in jargon and prone to sweeping generalizations that they are not very helpful for explaining their respective subjects. Although all of the authors either accept or show a fascination with assorted Neo-Marxian terminology in their analysis, the strong chapters are the ones that formally link their terms to quantitative evidence.

The Higgott essay, "The Dilemmas of Interdependence: Australia and the International Division of Labor in the Asia-Pacific Region," is a superb country study. It carefully documents the composition and patterns of growth in Australia's exports and imports and concisely states the principal problem facing Canberra's economic-policy managers: whether to continue with the country's current comparative advantage in raw-material exports or whether the long-run terms of trade make it more advantageous to shift focus and concentrate on high valued-added manufactures? As a high-income, well-educated, labor-scarce nation, Australia raises some atypical policy issues and Higgott deftly analyzes the trade-offs available, given the current factor proportions. He also notes the uncertainty that policymakers face when launching programs in sectors where local entrepreneurs have limited experience. Higgott concludes by commenting sensibly that analysts can disaggregate the economic choices into discrete areas but that political necessity (historical commitments or domestic and international constraints) often overrides and has an autonomy of its own.

The Mytelka chapter, "Knowledge-Intensive Production and the Changing Internationalization Strategies of Multinational Firms," is very ambitious and addresses a critical theoretical and policy problem. Mytelka attempts to identify how changing patterns in the generation and use of knowledge are affecting the global production process. Thus, in economic terms, she is trying to put human-capital theory on a par with other factors, such as changes in the composition of demand (i.e., the shift toward the service sector) and the structure of international capital markets.

The strength of the Mytelka chapter is that it directly links the theoretical parts with empirical evidence. Mytelka starts by showing the percentage increase of engineers and scientists in developed-country labor forces during the past two decades and by noting that research and development (R&D) costs are inversely correlated with an industry's product life cycle. She then shows how the decentralization and internationalization of knowledge generation has posed particular problems for multinational firms. Mytelka further documents various techniques firms have used to deal with the new circumstances: development of world products, flexible use of joint ventures, and links to university R&D capabilities. She concludes with two broad theoretical claims: multinational firms are coping with the rising costs and uncertainties of knowledge production by (1) attempts at cost minimization, and (2) efforts to control the markets in which the products are sold. The first claim is persuasively argued in the chapter; the second needs further elaboration. Overall, the significance of the arguments and the clarity of the exposition make this an impressive essay.

The Skak chapter, "The New International Division of Labor and East-South Relations," has a provocative and interesting theme. The central contention is that the European members of the Marxist countries' Council for Mutual Economic Assistance (CMEA) are in an increasingly competitive position with the newly industrializing countries (NICs). Both sets of countries are at "middle" levels of industrial development, are competing to import technology, and are anxious to get access for their exports in the industrial democracies. Although Skak's discussion occasionally overemphasizes the importance of East-South trade per se (only 1.5% of world trade in 1982), the basic issues raised here are quite significant. If the NICs continue to grow at a rapid rate while the CMEA countries proceed at a desultory pace, both the economic and political implications will be portentous. In a skillful manner, Skak notes that Soviet research analysts are very conscious of the same issues and have recognized that differing levels of economic performance among the developing countries and among the CMEA countries will shape future political options. This particular chapter

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would have benefited from more exploration of the reasons behind the NICs' success, but the central theme is compelling and these issues should be integrated into future general discussions of international political economy topics.

Each of the other chapters in this volume has fundamental problems. The theoretical overview provided by Caporaso touches on so many subjects that few of them are dealt with adequately. Although the tour through medieval and renaissance concepts is erudite, the discussion of how neoclassical economists deal with labor neglects production functions, investment theory, human capital, and relative price shifts. These omissions are combined with frequent vague phrases like "the cultural economy" and assertions that the Marxian approach to the global political economy is "open (even if coercively), integrated (even if hierarchically), and cosmopolitan." The concluding Caporaso chapter is, unfortunately, another literature survey rather than an attempt to unify the themes of the volume or to summarize empirical material.

The Junne chapter, "Automation in the North: Consequences for Developing Countries' Exports," addresses a very important topic, but its methodology is flawed. Although there is one citation claiming that robots in developed countries account for "about 40% of labor cost in manufacturing," one wonders to which sectors this applies and how the computation was done. Distressingly, Junne moves from claims that automation is sweeping developed country industries to other broad claims about policy, with little quantitative support for his argument. The Jacobsen chapter on Ireland is in a similar vein. A bewildering variety of comparisons are made, but the central contention that "peripheral postindustrializing" nations face a set of distinctive problems is not well demonstrated.

In sum, this volume is not a handbook and the chapters are quite varied, but it has several impressive essays that deal with key international political economy problems.

DAVID B. H. DENOON

New York University

Strategic War Termination. Edited by Stephen J. Cimbala (New York: Praeger, 1986. xvii, 227p. $37.95).

The idea of ending a nuclear war at the lowest possible level of death and destruction is clearly a very important concern. The professional and scholarly literature on preventing nuclear war is mountainous if often uneven and ideological, but public sector studies on terminating a nuclear war once begun are virtually nonexistent. This paucity is somewhat strange given that it has been administration policy for several years to end such a war at the lowest possible level of violence and on terms favorable to the United States.

This imbalance certainly reflects to a large extent the liberal intellectual bias of U.S. academic analysts of nuclear deterrence. In the standard view, deterrence simply cannot be allowed to fail; thus all energies must be devoted to maintaining the deterrent condition. Moreover, thinking about how nuclear war might be fought and ended in a manner short of an assured destruction cataclysm is often held to make nuclear war somehow more likely.

The problem with conventional thinking about nuclear dynamics that focus exclusively on deterrence or the first "campaign" of a nuclear war is that they omit serious consideration about how to end such a war. The authors of this compendium of original essays commissioned by volume editor and contributor Stephen J. Cimbala individually and collectively agree that this is a serious deficiency. As a number of them argue, the actual circumstances of nuclear hostilities, likely to be filled with the most incredible Clausewitzian "fog and friction" in the history of warfare, are hardly going to be conducive to rational, dispassionate analysis about how to end the war. The absence of serious consideration of war termination during the calm of peace could, indeed, make the escalation of nuclear war to an all-out exchange more likely, even a "self-fulfilling prophecy" as Barry Schneider puts it (p. 124). Beginning to fill this vacuum is the avowed purpose of this volume.

Cimbala has assembled an impressive roster of analysts to examine the problem. They include, in addition to the editor himself, William C. Martel, David W. Tarr, George H. Quester, Colin S. Gray, Leon Sloss, Paolo

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Stoppa-Leibel, Barry Schneider, Carl H. Builder, Robert Mandel, and Clark C. Abt. Cimbala gave each author considerable leeway both methodologically and substantively, and the result is a diverse set of perspectives on the problem. These range from examinations of the mechanics of nuclear war and its termination, the effects of different weapons systems and innovations on war and its ending, and notions about the costs and dynamics of nuclear war prevention and termination.

The various authors also present very different levels of optimism about the prospects of terminating a nuclear war. In one of the book's most revealing chapters, Colin S. Gray, long an advocate of the nuclear "war-fighting" —and even war-winning—school of deterrence, writes a very pessimistic chapter on the likelihood of stopping short of all-out exchange that should be read both by Gray enthusiasts and Gray detractors.

I found two problems with the book, one unavoidable, one not. First, the book is highly speculative, as indeed it must be in a world where there is absolutely no empirical evidence about the nature of nuclear war. A number of the papers attempt to make the study appear somehow scientific and substantive, but it is a hoax in the end. Thus, Martel draws elaborate alternative damage scenarios based on computer modeling in the opening chapter, and Mandel presents a large number of hypotheses about the war and its ending in his chapter. In the end, however, one is forced to admit that there are simply no facts on which to rely and build.

Secondly, in my view, the most important question about nuclear war—and thus about its termination—is why it might begin. In other words, if the purpose of all warfare is to achieve some political ends, what political purposes are adequate to initiate and conduct nuclear war? This question is central to deterrence because it defines the problems that require deterring. It is equally central to terminating nuclear war, since doing so presupposes either that certain goals are achieved or can be compromised. Unfortunately, it is a question that is nowhere seriously examined in this volume.

As Cimbala quickly admits in the preface, this is a first cut at, and not the last word on, the subject of terminating nuclear war. As a first effort in an area that has suffered from intellectual "benign neglect," it is a good and valuable effort. Its value is not so much in the answers it provides, however, as in the debate that it stimulates.

DONALD M. SNOW

University of Alabama

East-West Rivalry in the Third World: Security Issues and Regional Perspectives. Edited by Robert W. Clawson (Wilmington: Scholarly Resources, 1986. xxv, 348p. $39.00).

The Soviet Union in the Third World: Threat to World Peace? By Joseph G. Whelan and Michael J. Dixon (Washington: Pergamon-Brassey's, 1986. xlvii, 486p. $27.50).

We all know about animals created by committees. Both these volumes suffer a bit from the "committee" problem. The contributed chapters in the Clawson-edited volume are of somewhat uneven quality, and the book ends with a bump. A broad summing up at the end would have helped. The "committee" that influenced the character of the Whelan and Dixon book is the House Foreign Affairs Committee of the U.S. Congress. The book is essentially a collection of Congressional Research Service reports that were first pulled together and published as a congressional study.

So far as the Clawson-edited book is concerned, there are some excellent chapters. The collection resulted, by the way, from an April 1984 conference at the Lyman L. Lemnitzer Center at Kent State University. The best chapter of all is probably Boleslaw Adam Boczek's sensible and balanced consideration of the possibility of a "resource war" between the superpowers, in southern Africa, the Persian Gulf and in other Third World areas. Boczek concludes that such an all-out war—and Soviet victory in one if it should come about—are unlikely. The article by Louis J. Andolino and Louis R. Eltscher on Soviet power-projection capabilities is sober, competent, and well informed. It is not supportive, by the way, of the more extreme scare stories about Soviet naval superiority. Timothy W. Viezer's article on the Soviet invasion and occupation of Afghanistan is lively and interesting. It contains a few nuggets that may intrigue even those students who thought they

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had already read all there was to read about the 1979 invasion in Henry S. Bradsher's series of authoritative and masterful works. The introductory historical overviews that start the Clawson volume, written by Lawrence S. Kaplan and the editor, are well done if not substantively very startling.

The last third of the Clawson volume consists of "regional perspectives," or continent-by-continent analytical summaries of fairly recent developments. How one gets around the publication lag problem, incidentally, is hard to know. It is now over three years since these chapters were written. In any case, Robert I. Rotberg writes in his usual breezy and fact-packed style about the superpowers in Africa. He concludes that the Soviets are reeling from setback to failure in their efforts throughout the continent. He may be right, but his judgments in this regard could be a trifle premature. The other regional articles are less provocative and also somewhat less interesting.

The regional articles in the Clawson book cover some of the same ground that Whelan and Dixon traverse in their volume, *The Soviet Union in the Third World.* The Whelan-Dixon book's publisher, Pergamon-Brassey's, fudges in claiming that for speed of publication and economy of cost, "the author's typescripts have been reproduced in their original forms." What the publisher should have said is that the House Foreign Affairs Committee study, published in 1985, has been reproduced between Pergamon-Brassey's cardboard covers—typos and all. For example, one of the chapter subtitles is "Soviet Strategic Interests in America" (for *Africa;* p. 206).

In fairness to the publisher, the congressional study was never put on general sale, although copies were available for some time without charge from the House Foreign Affairs Committee and the Congressional Research Service. But for Gramm-Rudman, no doubt they still would be. The congressional study was in large part itself a republication of Congressional Research Service reports of 1983 and 1984, although Joseph G. Whelan updated his Latin American section for the 1985 congressional study.

So much for the odd provenance of the book. So far as substance is concerned, both Whelan and Dixon are clearly able, careful scholars and the study contains a wealth of useful material. Like many other government documents, it tends to have a lot of "on the other hand," and makes its points by selective quotation of academic scholars rather than by unvarnished assertion. This is not to say that the authors' views fail to come through. Whelan describes the USSR as having an "internal" empire (the minority communities in the USSR), a "contiguous" empire (the Eastern European satellites), and an "empire abroad" (India, Syria, etc.). The Indians would not be pleased. After characterizing the Soviet Union as a militarized society, Whelan quotes Arnold Toynbee: "Militarism breaks a civilization down . . . in destructive fratricidal conflicts. In this suicidal process the entire social fabric becomes fuel to feed the devouring flame in the brazen bosom of Moloch" (see pp. 444, 450).

Nevertheless, for all of their hard-line views, Whelan and Dixon are perceptive, extremely well informed and excellent students of Soviet policies and actions in the Third World. It might be worth closing with a contrast between two judgments, one expressed in the Clawson-edited book and the other presented in the Whelan-Dixon volume. In the Clawson-edited book, Gary R. Hess is discussing Soviet aid to Vietnam: "Considering the strategic advantages offered by the alliance with Vietnam, the economic and diplomatic costs are certainly reasonable" (p. 304). On the same subject, Whelan writes: "Vietnam is an expensive ally. . . . The Soviets pay a heavy price" (pp. 118–19). Interestingly, both books indicate that the Soviets are spending several billion dollars a year to support Vietnam; so "reasonable" and "expensive" are in the eyes of the beholder. As between the two judgments, Whelan's analysis appears closer to the mark.

NATHANIEL DAVIS

Harvey Mudd College

Foreign Policy Decision Making: The Influence of Cognition. By Martha L. Cottam (Boulder: Westview Press, 1986. xiii, 262p. $26.50).

The author of this book addresses three tasks. One is to provide a critical appraisal of previous research efforts to establish a link between the cognitions of decision makers and their foreign policy decisions. A second is to

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focus explicitly upon a model of cognitive processes that establishes this link. A third is to test the model of cognitive processes by examining the cognitive processes of U.S. decision makers in both an artificial and a natural setting. The results of these efforts are generally informative and provocative.

The generally informative aspect of the book is mainly in the discussion of the findings by cognitive psychologists regarding the processes of forming categories; making judgments about which objects fit into these categories and to what degree; and integrating into the process of judgment the context provided by the categories and the circumstances surrounding the object. For example, the author notes that this literature distinguishes among three types of categories: the "most abstract level," which provides little differentiation among objects; the "basic level," which "divides the environment into 'natural discontinuities' and therefore is the most cognitively efficient level of abstraction"; and the level of "'subordinate' categories whose members share attributes [that] overlap with those of other categories" (p. 37).

The author attempts to make the case that the "basic category" foreign policy decision makers use is "type of state." She asserts that seven types of states are identified by decision makers: enemy, ally, neutral, hegemonist, dependent ally of the enemy, dependent of the perceiver's state, and puppet of the perceiver's state (p. 50). The contents of each of these categories contains scripts that identify the expected behavior from states in each category and the appropriate responses by the decision maker; there is also a prototypical example of a state that fits into the category.

Cottam hypothesizes that these categories influence the foreign policy decisions directed toward a state that the decision maker judges as fitting into a given category—the closer the fit between a given state and the prototypical state in a category, the greater the influence of the scripts associated with the category, ceteribus paribus; that is, unless there is a disruption of this relationship by contextual influences, the expectation scripts will influence the decision maker's perception of the other state's behavior; the response scripts will influence the decision maker's policies toward the state.

The author offers evidence to suggest that decision makers do use "type of state" as a basic category. The data come from a questionnaire survey of U.S. foreign policy elites who were asked to group selected states by type of foreign policy and then label the groups. The author's analysis of the results indicates that these respondents grouped the states into all but one of her seven basic categories. In addition to this artificially generated evidence, Cottam also offers an interpretation in a natural historical setting of U.S. foreign policy toward Mexico. She argues that contextual influences caused many U.S. decision makers to categorize Mexico as a dependent state during the 1970s when it really had the properties of a neutral state. Consequently, the wrong scripts were activated in the minds of key decision makers, which led to policies that caused negotiations over natural gas imports to fail in 1977. They succeeded in 1979, on the other hand, because U.S. decision makers recategorized Mexico as a neutral state.

The provocativeness is a product of the author's willingness to take a position on several issues facing foreign policy analysts. For example, she attempts to locate the contribution of her cognitive process model within the cognitive tradition by contrasting it with shortcomings of the operational code and cognitive map models of the link between cognitions and foreign policy decisions. Cottam also asserts that her interpretation is a better explanation of U.S. foreign policy decisions toward Mexico during negotiations over the import of Mexican natural gas into the United States than explanations offered from a power politics, domestic politics, or a bureaucratic politics perspective.

STEPHEN G. WALKER

Arizona State University

Technology, Strategy, and Arms Control. Edited by Wolfram F. Hanrieder (Boulder: Westview Press, 1986. ix, 162p. $28.00 cloth; $13.95 paper).

This slim volume contains adapted scripts of lectures presented during 1984–85 to the Program of Global Peace and Security of the University of California, Santa Barbara. The areas of expertise of the nine contributors

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vary, but the political stance is uniformly moderate, favoring some form of United States–Soviet Union arms limitation. A further shared premise is that negotiated mutual reduction is the preferred means for arms control. There are neither Weinbergers nor Sagans, here.

The most critical stance is that of Richard Ned Lebow in "Assured Strategic Stupidity: The Quest for Ballistic Missile Defense," which cites Weinberger and relies on an earlier Union of Concerned Scientists publication jointly authored by Sagan, Lebow, and others. Lebow gives an overview of how a ballistic missile defense operates in theory, and criticizes the proposed Strategic Defense Initiative for undermining deterrence, increasing risks of use of nuclear weapons in crises, increasing stockpiles of offensive weapons to compensate for the percentage reliability of mutually deployed BMDs, abrogating the Anti-Ballistic Missile Treaty, and extending the arms race to new technologies and into space. This is the longest (24 pp.) and most extensively annotated entry and is more informative than several of the others in this uneven collection.

Herbert York and Paul Warnke each contribute a short historical overview of U.S.–Soviet negotiations and nuclear competition. The volume concludes with a brief moral argument for arms limitation, quoting extensively from others, by Catholic Bishop Thomas Gumbleton. These three papers provide context for more technical discussions in the volume but are clearly aimed at their young undergraduate audiences.

Chapters by Jonathan Dean and editor Wolfram Hanrieder discuss the European context of U.S.–USSR arms negotiations. Dean focuses on discussions between and within NATO and the Warsaw Treaty Organization in both Conference on Disarmament in Europe, and Mutual and Balanced Force Reductions talks and proposes an integrated regime for comprehensive negotiations on arms control in Europe. Hanrieder concentrates on the role of West Germany in U.S. management of NATO and criticizes the first Reagan administration for lessening the credibility of U.S. intentions to control arms and undermining West German faith in U.S. commitment to deterrence. He suggests a move away from the militarization of diplomacy but avoids specific policy recommendations.

Three essays discussing varied conceptual approaches round out the volume. Robert Hoover, in a well-organized and informative piece, contrasts presidential and bureaucratic politics paradigms for analyzing U.S. national security decision making, and suggests that actual policy-making, as in case studies of SALT and START, incorporates broader coalition politics. Kurt Gottfried raises probing questions on the impact of changing technologies, especially in deployment of Antisatellite Systems on deterrence and global security. He urges the scientific community to be active in the creation of an independent, "sound scientific advisory system" (p. 126). Ernst-Otto Czempiel argues for transforming the current East-West deterrence regime into one in which the conflict operates on a political rather than a military level. Defining the East-West competition in terms of antagonistic "isms," Czempiel musters historical evidence to advocate reliance on intergovernmental organizations for international conflict resolution.

The strengths of these essays are accessibility and breadth, ranging from relatively detailed discussions of ballistic missile defenses and negotiating stances to ethical appeals for global responsibility. The contributors do not differ significantly with each other on analyses of causes for the arms race or prescriptions for policies to pursue global security. A more comprehensive discussion of U.S. weapons deployment and the policy-making that underlies it is Paul B. Stares's *The Militarization of Space, U.S. Policy, 1945–84* (1985). A compendium with both more technically detailed and more politically diverse essays is *Weapons in Space*, edited by Franklin A. Long, and colleagues (1986). Each of these testifies to the inherent problems of the Hanrieder volume in gathering lectures designed for an undergraduate audience, regardless of the expertise of the lecturers.

CONSTANCE H. COLE

San Francisco State University

Small Is Dangerous: Micro States in a Macro World. Edited by Sheila Harden (New York: St. Martin's, 1985. ix, 212p. $25.00).

This slim volume embodies the report of a study group sponsored by the David Davies

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Memorial Institute of International Studies on the security problems presented by micro-states. Convened in 1984, the Davies Study Group was headed by Sir Peter Blaker, and included as members, inter alia, such distinguished scholars as Rosalyn Higgins, Malcolm Shaw, Donald Cameron Watt, and Jack Spence. The prestige of this group notwithstanding, the product of these deliberations may, at first blush, appear deceptively shallow; that is, the book, like its topic, is small and relatively short, containing less than 125 pages of formal text. However, a careful perusal of its contents leaves no doubt of its great utility and genuine value. *Small Is Dangerous* highlights a topic much overlooked in international relations; namely, the role which the multiplicity of little states have come to play in world affairs and, correlatively, the prospects their national existence and international activities present for political, legal, military, and economic disruptions in the world community. This project "aims to elucidate the difficulties caused by a state's lack of size and also to suggest ways in which the Governments of micro-states might seek to avoid or overcome these difficulties." Towards this end, the study "seeks to raise the consciousness of . . . decision-makers in the developed world . . . as to those policy measures which might be of practical value to micro-states in promoting their international and external security" (pp. 115–16). Given the resultant cogency of the analysis and perspicacity of its assessment and findings, the editor and authors of this report have succeeded laudably in meeting those ambitions.

The thrust of the analysis undertakes to examine "micro-states" within the international system. Specifically, the study addresses the potentially serious implications that their existence poses for international law and international security considerations, as well as the preventive measures designed to anticipate and short-circuit possibly destabilizing situations. As the study pointedly notes at the outset, "Clearly the world's micro-states, whether enjoying technical sovereignty or still classed as dependencies, have the capacity to cause macro-political havoc at the UN and in major world capitals" (p. 4). Two recent episodes plainly serve to underscore the validity of this concern that tiny polities may be the focus of titanic problems involving major actors: the Falkland Islands War in 1982 between Great Britain and Argentina; and the situation in 1983 on the island of Grenada, which subsequently led to an invasion by the United States.

Given this context of global realpolitik, the Davies Study Group analytically approaches the micro-state situation from a threefold perspective. First, the political character, regional affinity, and organizational affiliation of various micro-states are identified as they participate in the international system, within both the political and the legal dimensions. The number of micro-states obviously will vary according to whatever criteria are assigned for defining such a polity. In *Small Is Dangerous*, the determinant variable is population. *A micro-state* is defined as any state (or nonindependent island or other territory) that has a population of less than one million. Accordingly, the Davies Group suggests that some 43 states and 36 other nonindependent polities qualify for this designation. This may be the standard preferred by the United Nations, but something seems inherently wrong with a taxonomy that puts polities such as Botswana (the size of Texas), Namibia (twice the size of California), and Greenland (more than three times the size of Texas) in the category of "micro-states," along with Andorra (half the size of New York City), Saint Lucia (one-fifth the size of Rhode Island) or Nauru (with an area of eight square miles). If a flaw lies in the framework of the analysis, it rests in the resort to an apparently "quick-fix" standard for determining a "micro-" from a "normal," or a "macro-," state. Similarly, some difficulties also arise from the study's casual tendency to interchange micro-states with nonindependent micro-territories, but these may be explained away with a simple observation; very possibly in the near future, these latter polities will be independent as well. In any event, these criticisms do not impinge upon the validity of the study's arguments, nor do they undercut the value of its recommendations.

The second perspective treats the intervention problem confronting micro-states. That is, the international legal implications associated with the norm of nonintervention are assessed, as well as the need for maintaining adequate security forces in micro-states—not only to preserve internal stability but also

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to deter possible intervention by external forces. This portion of the study merits especially high marks. The Davies Group supplies a cogent, coherent examination of the place that international law holds for micro-states, particularly with regard to the processes of self-determination, decolonization, human rights, the law of the sea, intervention, and the threat posed by mercenaries. Appreciating the obvious vulnerability of micro-polities, much of the analysis appropriately focuses upon the scenario of external intervention into the affairs of micro-states and the legal implications that would arise from such an episode. In the same vein, the problems of and responsibilities for maintaining sufficient security forces are thoughtfully treated, with special emphasis aptly placed on the need for maritime security in policing those two-hundred-mile exclusive economic zones that have been proclaimed off the coasts of many micro-states.

The third and perhaps most valuable perspective of the study rests in its prescriptive recommendations. The study supplies several pragmatic suggestions for the "prevention, anticipation and cure" of factors that might contribute to fomenting political instability in micro-states. As determined by the Davies Study Group, the critical consideration most likely to affect political stability in micro-states is couched in the need for economic development. It is in this realm that Western trade and enhanced economic assistance is likely to have its greatest influence in stabilizing the political environment in micro-states. In this regard, three very useful "annexes"—some 75 pages long—are appended to cover the regions of southern Africa, the Caribbean, and the Pacific. Each annex supplies a country-by-country analysis of the micro-states in the region, treating matters such as those problems particular to that area's instability, the contemporary political situation of the micro-state, the prospects for national stability in light of possible sources of stress, and the potential for regional security services.

During the 1960s, Kurt Schumacher popularized the notion that "small is beautiful." As this study makes clear, however, when it comes to polities participating in international politics, small is also weak, fragile, vulnerable, and impotent. Moreover, small can be risky and dangerous—not only for the people and government of a micro-state but also for the international community at large. The clear message gleaned from this study is that micro-states ought not to be taken for granted, nor should they be dismissed as mere insignificant political minutiae. From little sovereign acorns mighty oaks of conflict may grow. There is much food for political thought here. Accordingly, the Davies Study Group has performed a valuable intellectual service in making governmental policymakers, as well as interested scholars and the general public, more acutely aware of this long-neglected dimension of international politics.

CHRISTOPER C. JOYNER

George Washington University

United States–Thailand Relations. Edited by Karl D. Jackson and Wiwat Mungkandi (Berkeley: Institute of East Asian Studies, 1986. 332p. $20.00).

The United States and Thailand: Alliance Dynamics, 1950–1985. By R. Sean Randolph (Berkeley: Institute of East Asian Studies, 1986. 245p. $15.00 paper).

These two books are part of a series of research papers and policy studies evaluating United States–Thailand relations during the tumultuous and uncertain period since the beginning of the Cold War. They add much to the literature in this gradually expanding field and they provide a wealth of information on new trends in this "special relationship."

Sean Randolph's study consists almost entirely of a chronological description of U.S. official relations with Thailand. A brief coverage is provided for "the early years" when U.S. traders and missionaries went to the Thai kingdom in search of profit and converts. Randolph relates the role of U.S. diplomats in opening official relations with King Rama III in 1833 and in assisting in the abolition of extraterritoriality in the 1920s. He covers the crucial transitional period just after World War II, when the United States replaced Great Britain as the dominant Western power involved in Thailand's quest for security in a new era of international politics.

The major chapters in Randolph's work explain the continual expansion of U.S. military

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and economic aid in Thailand after 1950, which resulted in the establishment of an "unsinkable aircraft carrier" by the mid-1960s, from which the United States launched many of its air attacks against North Vietnam. During this time the Southeast Asia Treaty Organization (SEATO) was formed for the "principal purpose" of guaranteeing the security of Thailand (p. 31). The gradual withdrawal of U.S. military forces from Vietnam under the Nixon Doctrine and the trauma of the fall of South Vietnam to Communist control in 1975 caused the Thai government to move away quickly from its close relationship with the United States and establish friendly contacts with regional powers such as Communist China.

In considerable detail Randolph covers the shift of Thai diplomacy from reliance on SEATO to the nonmilitary regional organization set up as the Association of Southeast Asian Nations (ASEAN). He also discusses Thai efforts to gain new commitments to its national security through economic development, trade, and investment rather than through military alliances. This pursuit was modified somewhat by the Vietnamese conquest of Cambodia in 1978–79, which placed the third largest army in the world on the Thai eastern border and revived a need for renewed assurances of U.S. military support for Thai national security.

The great strength of Randolph's work is the meticulous care in researching a vast variety of official and nonofficial sources in both the United States and Thailand. This work is very likely the most exhaustive study of Thai-U.S. relations yet published. The author gained information from numerous U.S. government documents as well as congressional hearings and reports. He used Thai government documents and English-language newspapers published in Thailand. An invaluable source was open and confidential interviews with past and present U.S. and Thai officials in Bangkok and in the United States. Randolph likewise relied on major scholarly books and articles on U.S.-Thai relations from approximately 1950 to 1984.

The only shortcoming in Randolph's book in the opinion of this reviewer is the lack of analysis and interpretation. The book is almost entirely a historical description, which is very well done but leaves the reader wishing for more "instruction" and "explanation" of some of the underlying meanings of this wealth of information on U.S. relations with a key country in Southeast Asia. What, for example, did the United States really learn about limitations on its military power in shaping the foreign behavior of a distant and friendly ally? What did Thai leaders learn about the continual need to focus on their own national interests? What were some of the specific influences of the vast U.S. presence in Thailand that have assisted the evolution of the Thai political system toward a "semidemocracy," where a new type of military leadership shares political power with a Western-trained body of technocrats, businessmen, and elected representatives?

The larger volume edited by two political scientists (an American and a Thai) consists of 22 chapters on a variety of topics on U.S.-Thailand relations ranging from history, culture, economic development, science and technology, trade and investment, politics, regionalism, and the role of the great powers on Thai security. The authors consist of diplomats, scholars, advisers, and businessmen from both countries. The collection ends with short but thoughtful essays on "future prospects" of U.S.-Thai relations by Thanat Khoman, a former foreign minister in Thailand, and Leonard Unger, a former U.S. ambassador to Thailand.

The chapters in the Jackson-Mungkandi work overlap in some areas with the historical coverage by Sean Randolph's book, but in many areas they provide more detailed and specialized information. The two volumes can very profitably be read together. The Jackson-Mungkandi book is especially strong in explaining the new role of the United States in aiding Thailand in its pursuit of an indigenous science and technology as well as its capacity to engage in research and development in advancing its economic and industrial development (pp. 69–72). Several of its authors stress that the time for relying on imported technology is over. A new Ministry of Science, Technology, and Energy (MOSTE) has been established. Thailand is gradually moving along the path pioneered by South Korea, Taiwan, Hong Kong, and Singapore toward the status of a "newly industrializing country." It is placing great stress on removing "income inequities" and reducing rural poverty.

One reassuring conclusion that emerges

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from both of these excellent books is that U.S.-Thailand relations continue to be strong and friendly in spite of numerous strains and challenges over the past 35 years. In some respects this "special relationship" becomes more stable and more mature as both countries recognize the different roles of a global superpower and a small regional power. The role of military force is still significant in the policy-making processes in both countries, yet it is more carefully balanced with constructive interactions in economic assistance, trade, investment, education, and cultural exchanges. The ASEAN regional organization has become one of the most active and healthy channels in fostering U.S.-Thailand relations. These relations face many challenges but a cautious optimism is very likely warranted. The two studies published by the Institute of East Asian Studies at Berkeley have provided a rich source of information and many useful insights to assess this important relationship in the future.

FRANK C. DARLING

Principia College

Psychology and Deterrence. By Robert Jervis, Richard Ned Lebow, and Janice Gross Stein (Baltimore: Johns Hopkins University Press, 1985. x, 270p. $27.50).

> There is no scientific literature on deterrence that begins to compare with, say, the literature on inflation, Asiatic flu, elementary reading, or smog.
> —T. C. Schelling, *The Strategy of Conflict*

A quarter of a century after Thomas Schelling lamented the absence of a scientific literature on deterrence, there has been a breakthrough in both quantity and quality of scholarly product in this area. *Psychology and Deterrence* is in the mainstream of a burgeoning literature that addresses the adequacy of deterrence as theory and as a guide to action. That literature now includes seminal works by Alexander George and Richard Smoke (*Deterrence in American Foreign Policy*, 1974), Glenn Snyder and Paul Diesing (*Conflict Among Nations*, 1977), as well as Richard Ned Lebow (*Between Peace and War*, 1981).

Psychology and Deterrence employs history to shed light on the most basic premises of deterrence theory and to test their validity across a variety of geographical, cultural, and temporal conditions. In the mainstream of a new wave of literature, the book stands out in the systematic use of psychology to explain deviation from the expectations of standard deterrence theory. The unifying theme of the volume is its disenchantment with deterrence both as a theory of nation-state behavior and as a strategy of conflict management. With good reason the authors believe that deterrence is inadequate as an explanatory theory. Empirical evidence indicates that neither leaders contemplating challenges nor those seeking to thwart such challenges act as the theory anticipates. The volume makes a less-compelling case, however, that deterrence fails as a prescriptive strategy. While there is some evidence presented that deterrence can provoke the very behavior it intends to prevent, there are too many cases either of deterrence success or where deterrence was irrelevant to justify rejection of deterrence as strategy. Indeed, the book's strength lies in its contribution to theory, while its weakness is in the evaluation of deterrence as a strategy of conflict management.

With respect to theory construction, there is some specification of conditions under which deterrence should hold, but the authors could have been more explicit in such theorizing before proceeding with empirical testing in different geographical, cultural, and temporal circumstances. There is an air of "empirical opportunism" in the testing of deterrence assumptions. While theoretical assumptions are treated systematically, the book is casual regarding research design considerations. In case selection, was there an attempt to maximize systematic variance in conditions under which deterrence assumptions should be valid? Was there consideration given to holding some variables constant while others could vary in the selection of cases? Since the book is silent about criteria for inclusion, one can only conclude that opportunity rather than design criteria reigned.

Research design aside, *Psychology and Deterrence* makes a major contribution to knowledge. Illustrative of that contribution are conclusions from Janice Stein's study in the volume. She concludes that "When deterrence

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held, leaders did see some prospect of bargaining, but when they chose to use force, they had no hope of diplomatic progress" (p. 58). Stein's conclusion implies that there should be a mix of accommodation and coercion, of reassurance and threat. Such an inference is consistent with the findings of George and Smoke, Snyder and Diesing, as well as Lebow. Since *Psychology and Deterrence* focuses on limitations of threats, however, a mix of positive and negative incentives per se is not addressed as a core concern of the book.

In addition to Stein's contribution, *Psychology and Deterrence* contains essays by Jervis, Lebow, Patrick Morgan, and Jack Snyder. There is consensus among them that a basic assumption underlying deterrence theory—rationality—fails to provide an adequate description of the way states actually behave. Nations are both more cautious and more prone to risk taking than deterrence theory would anticipate. The relative military balance is not as important as deterrence theory would predict. The military balance, even when validly assessed, is only one of several factors decision makers take into account in contemplating the use of force. Leaders also are influenced by (1) domestic and foreign political pressures that motivate them to use force; (2) a sense of frustration with the low likelihood of achieving their goals via nonviolent means; and (3) their assessments about future trends in the military balance.

A major conclusion of *Psychology and Deterrence* concerns the inner-directedness of decision makers. It is ironic that defenders spend so much effort attempting to reinforce deterrence by enhancing the credibility of their commitments when the target of these signals is preoccupied by an internally driven agenda. Even when the leaders in the target country do pay attention, moreover, they may distort or fail to comprehend the signals due to cultural, situational, and organizational barriers to valid interpretation. If leaders are inner-directed, however, they may not be subject to an escalation-via-miscalculation spiral that may be the downside of deterrence threats; that is, decision makers who are oriented towards their domestic agendas may be less subject to being locked into a threat-counter threat spiral.

As scholars create knowledge about deterrence, such knowledge apparently has little effect on national leaders. For example, during summer 1987, the United States was in the throes of making decisions about the augmentation of U.S. military power in the Persian Gulf. A naval buildup was justified on the grounds of the need to deter an Iranian attack against Kuwaiti and other gulf shipping. Undoubtedly, little attention was given to the psychology of deterring Iran by those who elected to reinforce deterrence. Another example of the gap between theory and policy regarding deterrence is in the area of arms control. While acknowledging that "deterrence is in the mind of the beholder—the Soviet Union," General Bernard Rogers (*New York Times*, 28 June 1987), former Supreme Allied Commander in Europe, focused only on U.S. military capabilities and ignored Soviet perceptions. So even when decision makers are aware that psychology is important in deterrence, they may not take steps to incorporate psychological findings into the process of deterrence.

RAYMOND TANTER

University of Michigan

Assessing Corporate Political Risk. Edited by David M. Raddock (Totowa, NJ: Rowman & Littlefield, 1986. xvii, 212p. $34.50).

During the late 1970s and early 1980s, political risk was a buzzword in corporate circles. The catastrophic financial losses suffered by many firms following the Nicaraguan and Iranian revolutions highlighted the need for good political analysis to guide foreign investment decisions. As a multitude of political risk analysts surfaced, all eager to offer their advice, the "profession" flourished for a few years. But it also suffered through an identity crisis as there was no consensus as to what "political risk" encompassed, how it should be analyzed, or, most important, who even qualified as a political risk expert.

Almost a decade later, the political risk field is still struggling for an identity. *Assessing Corporate Political Risk*, edited by David M. Raddock, reflects this struggle. Overall, it is an interesting collection of articles on a broad range of issues related to political risk. Among the contributors to the chapters in the volume are political scientists, area specialists, a

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former United States ambassador, a business professor, and an insurance expert. As would be expected from this diverse group of authors, perceptions of political risk vary greatly.

After a good general overview by Raddock on the types of domestic factors that might affect political risk in a given host country (e.g., regime legitimacy, leadership succession, ethnic conflict, civil disorders) and a very brief chapter, also by the editor, on financial and economic risk (e.g., debt service ratio, current account, currency competitiveness) there are five chapters by country specialists on political risk in the Soviet Union, China, Venezuela, Saudi Arabia, and Australia. The next two chapters present two different methodologies for forecasting political risk, each of which has been in existence for some time. One involves the "bootstrapping" approach, which attempts to "model the hidden cognitive process of the individual decision-maker(s)" (p. 157). The other depicts the "prince" system of political forecasting, which relies heavily upon expert opinions for identifying the key actors influencing the political climate in a given host country. The final two chapters deal with managing financial risk and political risk insurance.

With such a plethora of topics, Raddock is quite correct in stating that some of the chapters "can stand on their own and serve as examples for economists and political scientists to demonstrate how their own formal disciplines can be bent and interwoven to have a direct impact on bolstering profits in the business world" (p. 199). However, it is questionable whether he has succeeded in establishing "a theoretical interdisciplinary foundation for country risk analysis" (p. 199).

For such a foundation to be laid, there would need to be explicit theories, or even pretheories, of political risk proposed. This would entail identifying a series of testable propositions linking the many different variables that can directly or indirectly lead to political risk. A conceptualization of the varying effects that host government policies and actions, host societal developments, and events in the external environment can all have upon the formation of political risk would be necessary. While Raddock does include a descriptive chapter on host domestic factors, these variables are not explicitly linked to a theory of political risk.

For political risk to develop into a full-fledged discipline, there will also have to be an effort to build cumulative knowledge. That, however, goes against the understandable needs of the corporate planner who has to know, sometimes under a deadline of one or two weeks, what the political future of a given country is likely to be. This tension between the immediate applied side of political risk analysis, and the more long-range need for scholarly work remains a central problem for the profession. In that respect, Raddock's introductory statement that he has "drawn little from the field of political risk analysis and strategic planning because none of the books [or] articles . . . has more than skirted the subject or told part of the story" (p. xvii) does not help in the effort to build a discipline, and it much too quickly dismisses all previous work done in political risk analysis.

Nevertheless, this book will be of interest to those who want to become generally acquainted with political risk, and for those corporate decision makers who may want an overview of some of the types of political factors that may affect foreign investment.

JEFFREY D. SIMON

The RAND Corporation

Revolutionary Iran: Challenge and Response in the Middle East. By R. K. Ramazani. (Baltimore: Johns Hopkins University Press, 1986. 311p. $27.50).

Since the taking of U.S. embassy personnel as hostages in Teheran in November 1979, Iran has been identified as one of the main planners, financiers and supporters of international terrorism against the United States. The U.S. foreign policy establishment has conducted a concerted campaign, bordering on paranoia, to depict Iran's rulers as lunatic, fanatic, irrational, and dangerous. Paranoia in the conduct of policy, Richard Hofstadter argued, creates a paradox as it moves the policymakers toward "the imitation of the enemy," real or perceived. Thus, if Ayatollah Khomeini calls the United States the world's Great Satan, the Reagan administration considers Iran the Middle East's Satan. Consequently, U.S. policy, at least publicly, has been based on a rigid containment of Iran. A comprehensive and refreshing study of the foreign policy of the Islamic republic in the Middle East between

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1979 to 1986, *Revolutionary Iran*, among other things challenges the wisdom of the containment policy.

The book comprises five parts with 15 chapters. In part 1, Ramazani reasonably argues that instability in the gulf countries, as in other parts of the Middle East, cannot be analyzed as a conspiracy planned by the bearded mullahs in Iran's mosques. The abysmal failure of the gulf's conservative monarchies to "meet the mounting demands of their people for a better standard of living, and for social justice and political participation" makes them susceptible to the agitation of those who seek to disturb the status quo, including the Shi'ites (p. 53).

It is important to recall that although in many countries the Shi'ites constitute a large portion of the population, they rarely share power with the existing rulers. The chiliastic overtones of Shi'ism coupled with the demonstration effects of the victory of Shi'i fundamentalists in Iran have energized the discontented Shi'ites in all of the Middle East to become the harbingers of radical change and a source of instability.

Part 2 focuses on the causes and consequences of the Iraq-Iran War, and the role foreign nations, especially the gulf countries, play in that devastating conflict. Among the more interesting issues discussed is the Israeli sale of arms to Iran in 1984. The contention that the sale was "a one-shot deal," and that "it is possible that the deal never had the approval of high-level Iranian authorities" is debatable (p. 82). Moreover, revelations from the congressional hearings on the Iran-Contra affair seem to show extensive Israeli involvement in Iran's desperate attempts, authorized by Iran's senior officials, to buy weapons from 1985 to 1986.

Part 3 concerns Iran's relationship with Israel, Egypt, and Lebanon. Iran has rhetorically called for the eradication of the Jewish state. But there has been only one confrontation between the two nations: in retaliation for an Iranian attack on Israeli headquarters in Lebanon in which 29 Israelis were killed, the Israelis raided the headquarters of Iran's Revolutionary Guards in Baalbek and killed 23 individuals (p. 157). Ramazani concludes that because of the common bonds between them, Iran's hostility toward Israel will eventually be tempered (p. 160).

Part 4 deals with Iran's oil policy and the role of oil in the Iraq-Iran War. Ramazani shatters the perception, perpetuated by the U.S. media, that Iran has become a menace to free navigation in the Persian Gulf. It is, he rightly tells us, in Iran's national interest to keep the Strait of Hormuz open, for, unlike Iraq, Iran receives a substantial portion of its imports and exports through the gulf's warm waters. It was Iraq that started the tanker war in 1984 and has been the real threat to free navigation in the gulf.

In part 5, Ramazani suggests that in every major issue "including the war with Iraq, Iranian policy has consistently contained elements of self-restraint, pragmatism, and even helpfulness" (p. 237). The accusation that Iran has engaged in terrorism is based on "circumstantial evidence" (p. 45). Iran played no direct role in the bloody explosions in Kuwait in 1983 nor in the attacks on the U.S. marine headquarters in 1983 in Lebanon that resulted in the death of 241 Americans (pp. 189–90). Iran was also not involved in the hijacking of the TWA flight from Athens to Rome by the Lebanese Shi'ites in June 1985. In fact, Iran, as Lt. Col. Oliver North has testified in the Iran-Contra hearings, played a significant role in resolving that crisis (p. 191). But the Reagan administration was not forthcoming in publicly admitting Iran's contribution (p. 193). Thus, Ramazani proposes that the containment of Iran should be tempered—a wise policy that the Reagan administration surreptitiously pursued in 1985–86 (at least with two factions of Iran's elites) and is now under fire for because of the Iran-Contra scandal.

As for the Iraq-Iran War, because Iran might eventually emerge victorious, it would be perspicacious for the United States to avoid siding with Iraq, as it has recently done (p. 239). The decision by the United States to reflag and escort the Kuwaiti ships neglects Ramazani's prudent proposal.

Finally, Ramazani concludes that the "revolutionary regime's bark has been worse than its bite. Its rhetoric more strident than its actions" (p. 237). He provides persuasive evidence to substantiate this claim but does not fully explain the possible causes of this discrepancy.

In my view, in the postrevolutionary power struggle, the fundamentalists have consistently used anti-U.S. slogans, among other strategies, to mobilize support from the lower classes and

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to weaken and defeat the moderates accused of pro-Americanism. In managing Iran's military and economic systems, however, they have recognized the depth of Iran's dependency on the United States, a dependency created because of Iran's close ties with Washington in the post-World War II period. Thus, the fundamentalists have been torn between the requirements of a dependent system that they inherited from the Pahlavis and the rewards of verbal attacks on "U.S. imperialism" to mobilize support for keeping the revolutionary spirit alive. The net result has been secret negotiation with the United States to satisfy Iran's dependency and public condemnation of the Great Satan for internal consumption.

Enlightening, provocative, and well argued, this book is a must for students of the Islamic Revolution and a valuable text for courses on the Middle East.

MOHSEN M. MILANI

University of Southern Florida

Indian Security Policy. By Raju G. C. Thomas (Princeton: Princeton University Press, 1986. xiv, 312p. $37.50).

Since the late 1970s, India has undergone its most impressive military buildup since independence. Defense spending has increased almost 200%. A series of new arms deals have modernized the armed forces with about $20 billion in new equipment. In his new book, Raju Thomas shows that Indian governments, like the Reagan administration, find it easier to increase military spending than to explain what all the new capability is supposed to do.

Thomas, professor of political science at Marquette University, previously wrote about Indian security politics in his 1978 book, *The Defence of India*. It remains the best introduction to the subject. His new volume recapitulates and builds upon this proven foundation. Many of the new arguments were rehearsed in a series of journal articles, so specialists will not find a lot of fresh meat here. But for newcomers, Thomas sets just the right tone. Indeed, the most significant aspect of Indian security policy-making may be how little it has changed over the years.

India faces several serious security problems, including civil strife, continuing worries about China and Pakistan, as well as the regional presence of the superpowers. To cope with this situation, New Delhi has assembled the world's fourth largest military establishment. Yet 40 years after independence, the government has never issued a defense white paper or a posture statement laying out its strategic objectives or long-range goals.

India's security policy-making is a curious mixture of the gyroscopic and the chaotic. Thomas's portrait of the armed services and military industries reveals a model of bureaucratic stability. As elsewhere in the world, big defense programs take on a life of their own. Helped by big budgetary increases, they advance toward popular goals. The government-owned military industries pursue self-sufficiency in modern technology. The army maintains its 30 divisions, while the navy grows towards three carrier groups and the air force modernizes its 45 combat squadrons.

Although there is a healthy public debate on defense issues, it has not fostered serious official investigation of these fundamental defense programs. Are they the best way to assure Indian security? Doubts are expressed, but not in the Congress party-controlled parliament. Nor is there serious questioning of the 70-20-10 rule for apportioning funds between the army, air force, and navy. The budgetary formula is the closest thing to a national security doctrine that India has.

At the cabinet level, however, decision making can be unpredictable, even panicky. Thomas's natural caution precludes direct criticism, but the picture he presents is unambiguous. India has become a major military power, but it still relies on the decision-making apparatus of a small and isolated country. Institutional arrangements tend to be unrefined. They often go unheeded. Civilian leaders long ago grew accustomed to disregarding military opinion. Major decisions usually are made by the prime minister personally with a handful of close advisors.

Policy is guided by the shibboleths of nonalignment, development, and self-reliance. But these provide little practical guidance. In lieu of long-range goals or strategic planning, decisions often reflect crisis reactions. For example, despite achieving regional supremacy by the time of the 1971 war, Indian officials were acutely sensitive to any Pakistani arms acqui-

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sition. When Washington offered Islamabad $1.6 billion of new arms in 1981, including 40 F-16 fighters, the Indian press pictured skies black with the planes. New Delhi forgot about its own $4 billion of recent warplane purchases and sought new aircraft from several suppliers. Reflecting this climate, Thomas joins Indian leaders who refuse to believe that their new Mirage 2000s and MiG-29s are comparable to Pakistan's older U.S. aircraft. For insurance, New Delhi wants superior quality and greatly superior numbers. Should Pakistan acquire AWACS or test a nuclear device, India would no doubt follow a similar course.

The obscurity of Indian security policy-making leaves one hungering for more insight. Why have defense expenditures increased so much? In reaction to Pakistan? In pursuit of regional hegemony? What of the consequences of Soviet involvement in Afghanistan? What of the future direction for the nuclear weapons program? Thomas sticks close to the official debate. He has surprisingly little to say on these questions.

One strength of the book is its discussion of internal security problems. Civil strife and sectarian violence have grown more serious, with secessionist movements in several parts of the country. Thomas shows how India has built a panoply of paramilitary forces to cope. Like the professional military, he hopes the armed services can be kept out of these affairs.

AARON KARP

Stockholm International Peace Research Institute

Multilateral Development Diplomacy in UNCTAD. By Thomas G. Weiss (New York: St. Martin's, 1986. xviii, 187p. $30.00).

In prior works, Thomas G. Weiss has argued that international organizations such as UNCTAD (United Nations Conference on Trade and Development), perform a valuable role in the international politics of agenda formation. In this book, Weiss concedes that although UNCTAD's group bargaining system has contributed greatly to solidarity among developing countries and launched a multilateral development dialogue between North and South, it has outlived its usefulness and has become a barrier to a more equitable international economic order. Thus, this book asks, how can multilateral development diplomacy in UNCTAD be restructured to make the most of existing and future opportunities in negotiations between North and South?

Because UNCTAD has failed to translate general principles into specific policy outcomes through negotiations, Weiss begins his analysis with a scrupulously fair, but frank, insider's account of where UNCTAD has gone wrong. Weiss acknowledges that not all the blame rests with UNCTAD's organizational and procedural shortcomings. UNCTAD's recent efforts to improve the lot of the developing world is also constrained by slow world growth, Western intransigence, and the often flawed domestic policies of developing countries. Nonetheless, Weiss demonstrates that the UNCTAD system of group bargaining, which pits a huge block of diverse undeveloped and developing countries against the developed world, impedes successful negotiations as well. Specifically, the current group arrangement leads developing countries to prepare excessive and inclusive lists of demands, adopt radical positions, and offer inflexible, one-sided, and often ill-prepared initiatives. These aspects of the current group system contribute to unfocused negotiations that promote southern solidarity while hiding real differences in the developing world, and, more importantly, impede progress toward global redistribution of wealth.

Weiss offers an imaginative, yet realistic, "alternative image" of multilateral development diplomacy as a way out of the current stalemate. The essential elements of his alternative approach include: (1) flexible coalitions of countries based on interests rather than ideology; (2) concentration on specific issues on which there is some room for compromise and some possibility of building further agreements; and (3) proper analytical and political analysis—spearheaded by a more technically competent and less partisan UNCTAD Secretariat—before negotiations.

The author's recommendations are sound and founded on lengthy personal experience. Moreover, Weiss's insights reflect a balance of pragmatism and optimism that is indeed rare. Nonetheless, the question the author poses is

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unfortunately limited to what can be achieved *in* UNCTAD, and his analysis is unnecessarily confined to his own and others' observations *about* UNCTAD.

Equally important are the questions whether multilateral development diplomacy can best be restructured inside or outside the UNCTAD system and a consideration of the relative merits of alternative multilateral, bilateral, and plurilateral approaches to development diplomacy. For example, many developing countries, recognizing that they need the protection of multilaterialism and most-favored-nation treatment, have already subordinated group politics in UNCTAD for a more active role in the preliminary stages of the current round of General Agreement on Tariffs and Trade negotiations. Weiss does not address these issues, however.

Similarly, the author's answer to UNCTAD's problems draws only from his first-hand knowledge and from the prior literature on UNCTAD. Although multilateral development diplomacy may be a recent phenomenon, it is disappointing that in assessing UNCTAD's shortcomings Weiss brings to bear no lessons from prior or contemporary works on conference diplomacy, multilateral negotiations, organizational literature, or even world politics.

In short, this book offers an imaginative and reasonable answer to the question the author has posed. Ultimately, the author's contribution may be constrained by the declining relevance of UNCTAD to the future course of development diplomacy.

WILLIAM J. LONG

Columbia University

POLITICAL ECONOMY

Latin American Political Economy: Financial Crisis and Political Change. Edited by Jonathan Hartlyn and Samuel A. Morley (Boulder: Westview, 1986. xiii, 386p. $38.00 cloth; $16.95 paper).

The field of Latin American comparative politics has been rather dominated by various types of "political economy" approaches. Nevertheless, in a region tormented by crises of development, a slowdown in growth since the 1960s, and present financial chaos, urgent questions remain regarding the nature of the linkages between states and markets, political societies and productive structures, accumulation and legitimation. Political economy should be more than economics with a sprinkling of references to policies, and more than political sociology with a bias towards explanations rooted in classes and economic sectors. This book brings together a number of valuable contributions to a field in which controversy too often rages without evidence.

Hartlyn and Morley have organized these essays (many first presented to the International Conference on Models of Political and Economic Change in Latin America at Vanderbilt University in November 1983) into five comparative chapters that form the first third of the book, followed by eight case studies: Argentina, Brazil, Chile, Peru, Mexico, Colombia, Cuba, and Nicaragua. This makes the second two-thirds more useful for teaching and reference purposes than as an exercise in new theory building, or comparative theory testing. Some might have preferred the editors to establish a common time frame and theoretical agenda of questions to be addressed by the large number of contributors. The strategy seems rather to have been to survey as much of the region as possible (hence the inclusion of a socialist economy in the case of Cuba), and readers are left to draw their own conclusions from the case studies.

Perhaps the most stimulating ideas emerge from the comparative essays. Though they omit Uruguay, the editors' review of the performance of Bureaucratic-Authoritarian (BA)

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regimes convincingly disconfirms the view that authoritarianism leads to faster growth than democracy. They argue for a distinction between countries that have implemented pendular policy swings, and thus experienced stop-go cycles rather than sustained growth (Argentina, Chile, Peru), and those that have pursued more consistent policies and thereby grown steadily (Colombia, Mexico, Venezuela).

Albert Fishlow's comparative essay, while occasionally more econometric than many political scientists will appreciate, provides a very important critique of IMF stabilization policies. A particular merit of Fishlow's approach is that he includes various contrasting Asian cases in his study of adjustments to the oil shocks of 1973 and 1979. An essay by David Felix usefully details how BA regimes adopted very different economic policies, though what determined policy choices remains unclear. Brazil, in particular, committed so many *dirigiste* "deviations" that it bore little resemblance to the neoliberal experiments of the Southern Cone. The original BA model proposed by O'Donnell did not really predict the virulent neoliberal policies of the 1970s. Nevertheless, both types of policy—developmentalist and neoliberal—ended in "financial blowups," causing Felix to conclude with an intriguing sociocultural hypothesis. This is that free trade is less likely to produce sustainable growth in Latin America than in Asia because Latin American elites are far more thoroughly imbued with Western tastes. Import liberalization merely leads to an unsustainable boom in luxury imports, rather than a balanced process of competitive outward-oriented growth. This reviewer (a Latin Americanist) would be interested to hear whether Asianists find this notion plausible.

The country studies (most by nationals of the country under scrutiny) are almost all very strong and provide a lot of detail on the vicissitudes of economic policies and performance over the past decade. In a review of this length there is no room to mention all of them. Perhaps the most interesting is the chapter by Marcelo Diamand, which introduces English-speaking readers to the author's theory that the blame for Argentina's perplexing slide into relative underdevelopment may lie in the strength of its export agriculture. Argentina's very competitiveness in beef and grain led to a constant overvaluation of the exchange rate, much as oil bonanzas have done in other nations. This provocative hypothesis deserves further elaboration and testing. Those seeking a more explicit focus on the political roots of economic policy swings in what has been the worst case of "stop-go" in the region will want to supplement Diamand's account with the writings of other Argentines, such as Guillermo O'Donnell and Marcelo Cavarozzi.

The Brazilian case study by Lamounier and Moura focuses on the complex interaction between the military's decision to begin a political "opening" after 1974 and at the same time increase growth in order to get round the first oil shock. It is a convincing narrative. Whereas other chapters focus primarily on economic trends, the impressively synthetic essay by Manuel Antonio Garretón offers an elegant structural-historical account of Chile's political polarization and social mobilization in the 1960s, brief socialist experiment and subsequent neoliberal military dictatorship. In many ways this chapter complements Paul Cammack and Phil O'Brien's *Generals in Retreat* (1985), which includes a detailed essay on the Chilean economy under Pinochet by William Smith.

The chapter on Colombia, a comparatively stable democracy since 1958, by Revéiz and Pérez, provides some interesting contrasts to the instability of the other countries, and suggests that the secret of steady growth lay in the ability of certain producer groups to "capture" different state agencies. For those who see "state autonomy" as conducive to economic development this is a provocative finding. Readers are left wondering, however, why the authors currently perceive such a severe crisis and how they explain it. Ayala and Durán's discussion of Mexico makes a welcome effort to link policies before and after the oil bonanza to a discussion of the class forces acting through a hegemonic one-party regime.

Though this book appeared too soon to discuss the crucial antiinflation strategies recently attempted by fledgling democracies in Argentina and Brazil, it provides a great deal of stimulating discussion for those interested in the constraints economies and polities place on one another in a region of the world where extremes sometimes seem to be the norm. The sad fact is that while there is a lot of political economy written about Latin America by

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Latin Americans, policy-makers too often rely on imported theories under pressure from international agencies.

CHARLES GILLESPIE

Amherst College

Pipeline Politics: The Complex Political Economy of East-West Energy Trade. By Bruce W. Jentleson (Ithaca, NY: Cornell University Press, 1986. 263p. $29.95).

Pipeline Politics is an engaging, well-researched account of Western energy trade with the Soviet Union during the postwar era. The central analytic claim is that for the United States to have coercive power over the Soviet Union, both domestic and international constraints must be minimized. The former are characterized as a function of ideological divisions and group economic interests, the latter result from the divergent foreign policy strategies and economic interests of the United States and its allies. Jentleson proceeds historically, seeking to shed light on the ability of the United States both to obtain the cooperation of its allies and to use energy trade effectively to achieve instrumental objectives in its relationship with the Soviet Union.

The main contention with regard to alliance politics is that over time the intensity of conflicts has increased, while the sources of U.S. leverage have decreased. Consequently, the United States was more successful (though never fully so) in shaping alliance disputes in the 1950s and 1960s than in the 1980s. While perhaps not striking, this argument is nevertheless carefully documented and convincingly demonstrated, with an insightful emphasis on variations in prestige as a factor influencing the ability of the United States to shape alliance outcomes. Equally important, Jentleson shows that the divergence of interests underlying the 1982 pipeline dispute is a structural one, suggesting the possibility of further alliance conflict over this issue. Whether or not the United States, despite its relative decline, continues to possess sufficient leverage in absolute terms to determine alliance outcomes in conflicts of lesser political and economic salience than the 1982 dispute is a question yet to be answered. While not addressing it in detail, Jentleson's analysis suggests that the United States is likely to encounter great difficulty in obtaining support, except in areas where alliance interests are more convergent, for instance, CoCom-related trade.

Domestic constraints, like international ones, have become more pressing over time. Congressional opposition undermined the Nixon-Kissinger inducement strategy, while pressure from domestic economic interests helped to compromise the Reagan economic warfare initiative. In light of the breakdown of the Cold War consensus, and the failure of a new consensus to emerge, U.S. officials are destined to confront significant domestic opposition regardless of whether they opt for a trade inducement or trade restriction strategy. In general, *Pipeline Politics* goes further than most studies of economic sanctions in building the domestic politics of the sanctioning state into the analysis. This is an important contribution. The next conceptual step would be to provide systematic analysis of the resources and tactics available to administration officials in their struggle to overcome domestic constraints, an analysis parallel to that provided in the international context.

In assessing the impact of sanctions on the Soviet Union, Jentleson highlights the complexity of U.S. objectives, categorizing them as economic warfare, economic defense, deterrence, compulsion, and containment. This disaggregation, drawing upon the example set in recent scholarship on economic statecraft (e.g., David Baldwin's work), rightly reflects that objectives are rarely one-dimensional and consequently that outcomes cannot be characterized in simple terms of success or failure. The evaluation of U.S. efforts is sketchy at points, reflecting the difficulty of obtaining detailed evidence on the impact of sanctions, as well as that of drawing inferences from the evidence that is available. Nevertheless, the overall assessment—that the United States has never been overwhelmingly successful yet was relatively more successful during the Cold War than more recently—is persuasive. With regard to the future, "the prospect that any anti-Soviet sanctions may produce significant gains for American foreign policy in terms of any of its longstanding economic and political-instrumental objectives is exceedingly slight" (p. 245).

Pipeline Politics demonstrates the utility of careful historical research, grounded in a sensi-

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ble, not overly deterministic analytical framework. It draws upon existing concepts to yield insights on an important issue that, until recently, received relatively scant scholarly attention. It is a solid contribution to the literatures on international economic sanctions and U.S. foreign policy and deserves the serious attention of scholars in those areas.

MICHAEL MASTANDUNO

Dartmouth College

Wayward Capitalists: Target of the Securities and Exchange Commission. By Susan P. Shapiro (New Haven: Yale University Press, 1984. xix, 227p. $26.00 cloth; $9.95 paper).

In the past several years the crackdown by the Securities and Exchange Commission (SEC) on insider trading, financial fraud, and other abuses has dominated the financial news. The timely reissue of Shapiro's contribution to the Yale Studies on White Collar Crime offers a case study of the enforcement strategies of the SEC. The book's superiority over popular accounts of financial crimes is derived from its source material, which consists of interviews with the commissioners and staff of the SEC and confidential data from investigations conducted by the agency between 1948 and 1972.

Shapiro asserts that the vitality of capital markets depends on trust, but that the intangible nature of securities makes those markets vulnerable to fraud and misrepresentation. The thesis of the book is that the intelligence system employed by the SEC constrains the type of information that can be discovered and, hence, the enforcement actions that are undertaken. The book provides a taxonomy of the offenses policed by the SEC and descriptive statistics showing the most prevalent crimes at each stage of detection and prosecution. Though each form of illegal behavior is vulnerable to discovery, the SEC's detection methods have limited accuracy. Most of the SEC's actions are generated by investor complaints and by errors in required filings, a relatively passive source of intelligence. The SEC's typical enforcement action involves few victims and few offenders, with a median cost to the victims of $100 thousand. The book concludes that the SEC has little control over its enforcement program because of its "passive dependence on reactive intelligence" (p. 94) and claims that the SEC could be better organized to exploit information about law breaking.

An unstated public interest theory of regulation drives Shapiro's analysis. She regards the SEC as the "preeminent protector of the capital markets in this country" (p. 4) despite recurrent periods of lassitude in its history and its "skewed" enforcement policy. The possibility that some benefit privately from the SEC's enforcement policy and that beneficiaries may influence enforcement policy through the detection mechanism is never addressed. If enforcement is skewed because of the mechanisms of intelligence gathering, knowing the motivation of those who design and control the intelligence gathering apparatus is crucial for understanding agency behavior.

Furthermore, Shapiro examines the detection and prosecution record without questioning the ties between the agency's policymakers and the Congress and the president. The impact of political goals and the hierarchical structure in which the agency operates on the organization of intelligence gathering and enforcement is ignored. It is not surprising, for example, that the SEC relies on reactive intelligence if the agency serves as a barometer of interest group power for congressional or executive overseers. No reference is made to theories of regulation which explain patterns of agency policymaking and which may explain the organizational structure of administrative agencies.

Shapiro's interview and case study approach is embellished with statistics, but no hypotheses are offered or tested. In describing investigations by the SEC, the SEC's information sources, and the offenders and victims, she complements colorful anecdotes with a statistical breakdown of the offenses. Tables containing the summary statistics are unreadable, however, because of multiple classifications. Factor analysis is employed to show the common attributes of the different detection strategies. However, the only artifacts of the empirical procedure are graphs of the detection strategy loadings in factor space and a vague discussion of the results. It is difficult to leap from the textual discussion of results to the important conclusion that the detection strategies

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employed by the agency determine the cases that are prosecuted.

The usefulness of the book to political scientists and economists is limited because of its atheoretical approach to agency policymaking and hypothesis-free empiricism. *Wayward Capitalists* is, however, a rich description of financial crime and enforcement that may be useful as background material to a more rigorous study of the organization of administrative agencies.

MARK J. MORAN

Case Western Reserve University

Private Interest Government: Beyond Market and State. Edited by Wolfgang Streeck and Philippe C. Schmitter (Beverly Hills: Sage, 1985. x, 278p. $35.00 cloth; $14.95 paper).

This new volume in the Sage Series in Neo-Corporatism is a collection of papers that, with two exceptions, were first presented at a 1983 colloquium in Florence sponsored by the European Group for Organizational Studies (EGOS). The opening essay by Streeck and Schmitter argues that "the association" should be viewed—along with the community, the market, and the state—as "a distinctive fourth institutional basis of order"; examines theoretically the logic of "an associative model of social order"; and explains how and why elements of corporative-associative order have emerged in the form of "private interest governments" (PIGs!) in particular sectors, industries and policy areas of many polities. This piece, by far the most ambitious in the book and doubtless the one destined to be most widely read, thus places the traditional neo-corporatist paradigm within a broader theoretical context and at the same time clarifies its relevance to current public policy debates, presenting the "regulated self-regulation" that corporatist PIGs can offer as a strategic alternative to either direct state intervention or a neo-liberal restoration of the market.

As Streeck and Schmitter note in the preface, the 11 other contributions in the book "are empirical studies that, in one way or another, marshall positive or, as the case may be, negative evidence for the themes and hypotheses raised in the first chapter." Valid though that contention is, it must be said that most of the chapters address those themes only in an unsystematic, disjointed, and sometimes peripheral fashion. Since the authors approach their topics from a wide variety of perspectives and are preoccupied with disparate research agendas, readers concerned with the issues highlighted by the editors must be prepared to wade through or skip over debates on such extraneous matters as the "conceptual foundations of accounting" and the alleged failings of "orthodox interpretations" of Burns and Stalker's *The Management of Innovation.* Moreover, readers will be challenged at times to perceive the thematic coherence of essays dealing with six different sectors or issue areas (advertising, accounting, pharmaceutical price regulation, debureaucratization, milk marketing, and the voluntary sector) in national cases ranging from Britain (five chapters) to Holland (two chapters) and Austria and Switzerland (one chapter each). Only one chapter handles its topic comparatively (with an assessment of advertising self-regulation in Belgium, Canada, France, and Britain), while one other presents not the usual intensive case study but rather a typology of modes of governance in the various sectors of the U.S. economy. With such a motley collection of essays, the book could have been greatly strengthened by the inclusion of a concluding chapter drawing connections between the theoretical concerns voiced in chapter 1 and the disparate empirical case studies that follow. Alas, while the editors note in the preface that they originally intended "to close the volume with another essay reassessing the potential and performance of private interest government in the light of the empirical material contained in the collection," this project "had to fall by the wayside" given the press of publication deadlines and the fact that "a major intellectual effort" would have been required to deal with an issue that emerges as a "complex and thorny one, perhaps more so than was originally thought." Readers are thus left to pick up the pieces themselves.

What the editors do not mention is that a different type of organizational scheme might have enabled them to reach at least some narrower, more focused conclusions while also enhancing the volume's analytical punch. Disparate though the chapters are, all but two of them could have been organized into two coherent parts. First, the essays by Boddewyn,

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Willmott, Wilson and Butler, Hughes, Sargent, and Grant could have been presented as sectoral studies of "PIGs in Britain." Viewed comparatively, these chapters provide not only complementary perspectives on the multifarious uses of private interest government in the British case but also illuminate a point raised recently by Schmitter in another book (Wyn Grant, ed., *The Political Economy of Corporatism*, 1985): while the concept of *neocorporatism* has "contributed significantly to [a] revival of interest in the state," the empirical studies inspired by it have provided reminders that "*the* state" is in fact "an amorphous complex of agencies with ill-defined boundaries, performing a great variety of not very distinctive functions." As the relevant chapters of this volume show, *the* British state is a remarkably complex, fragmented set of actors ranging from the Department of Health and Social Security to the Charity Commission. Second, the essays by Traxler, Farago, Grant, and van Waarden (and part of the one by Hollingsworth and Lindberg) could have been packaged as "PIGs in the Dairy Industry: A Comparative Perspective." These chapters together raise an obvious, fascinating question: how is one to explain the emergence of a strikingly similar mode of private interest government in the dairy sectors of polities as diverse as Austria, Switzerland, Britain, Holland, and the United States?

The strength of this volume is that it raises such questions and, in the editors' words, offers "rich material for future cross-national comparisons aimed at an improved understanding of the social and institutional conditions for the emergence and functioning of private interest government." Its central weakness is that it generally stops short of making the comparisons and providing the answers.

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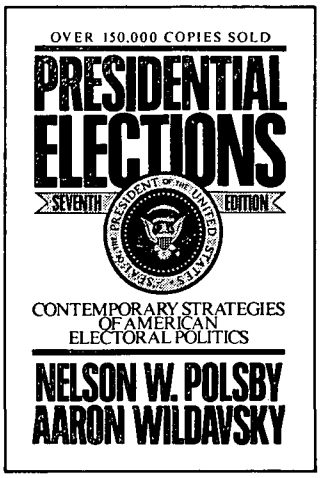

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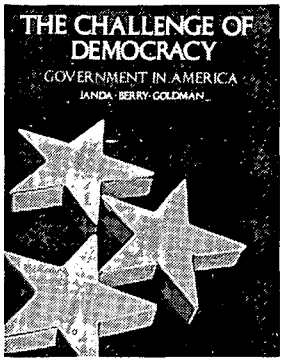

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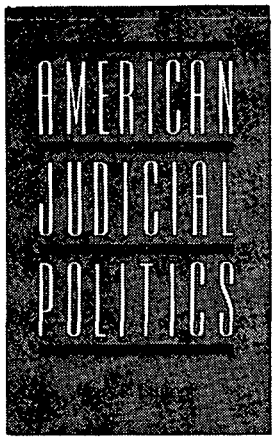

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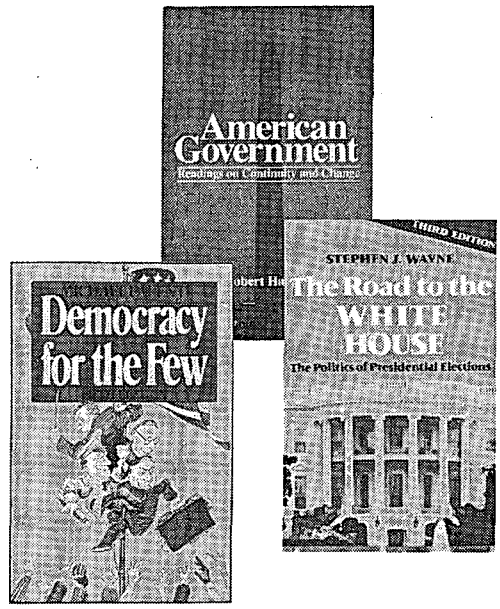

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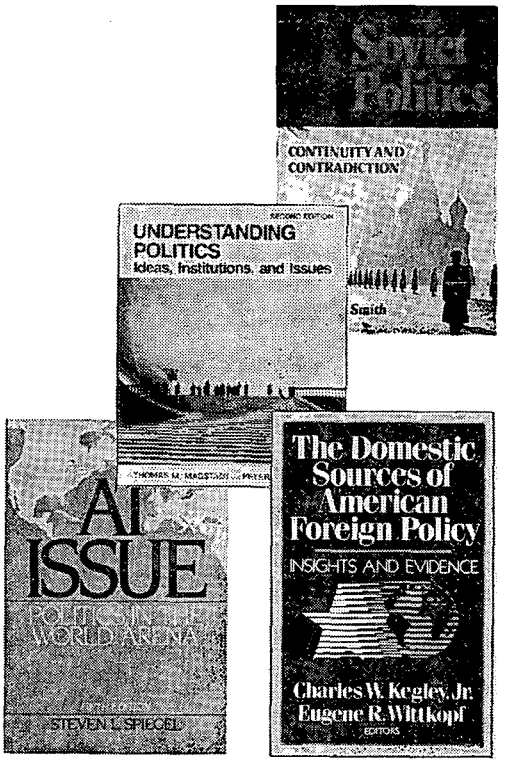

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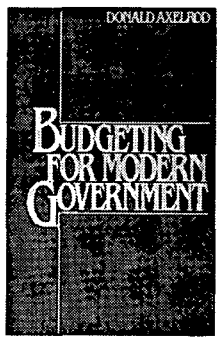

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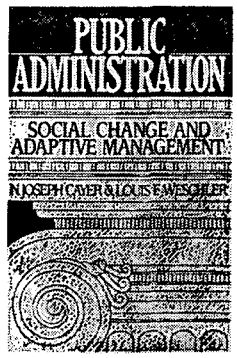

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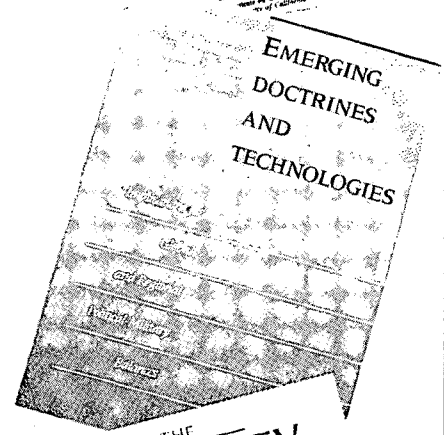

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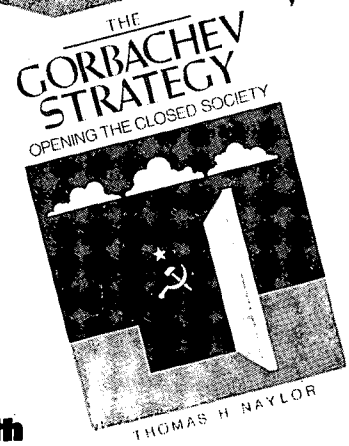

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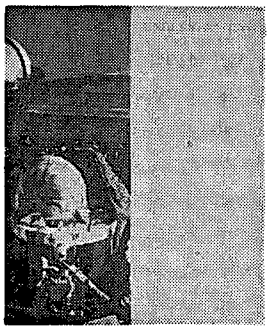

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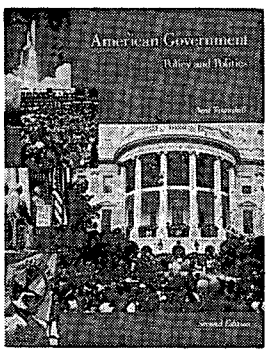

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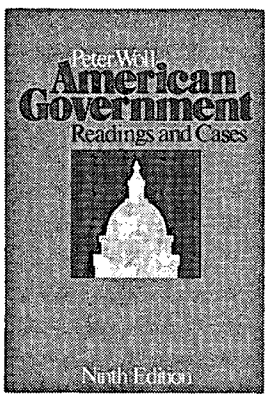

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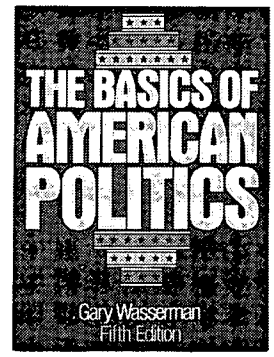

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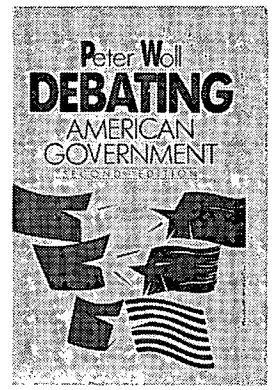

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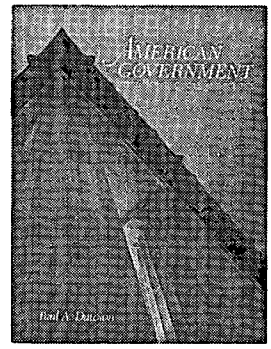

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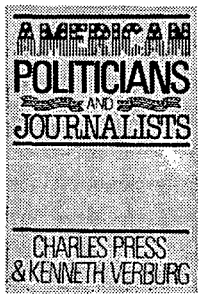

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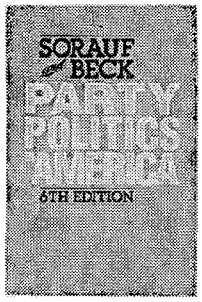

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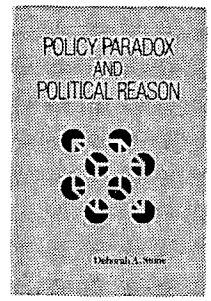

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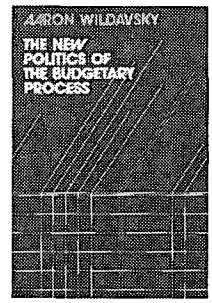

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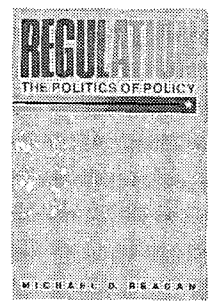

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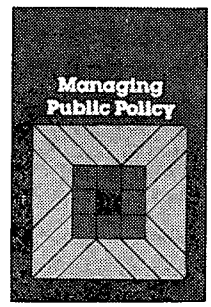

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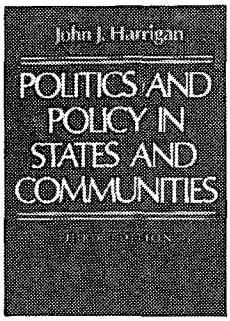

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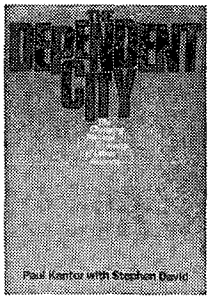

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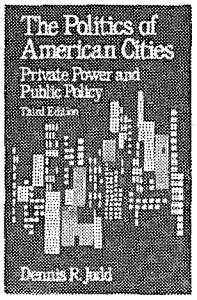

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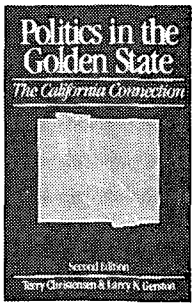

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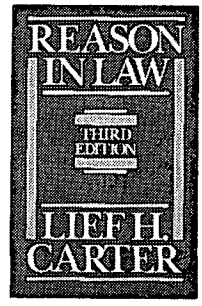

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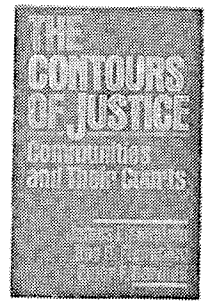

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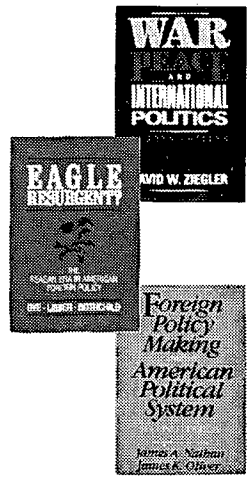

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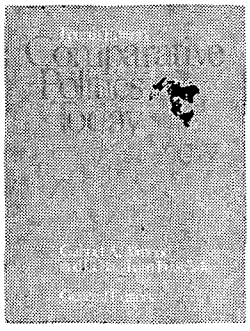

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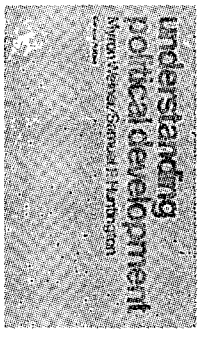

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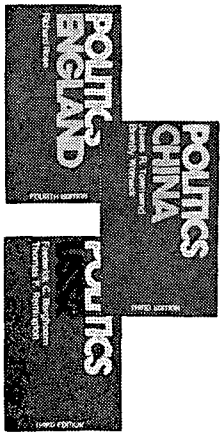

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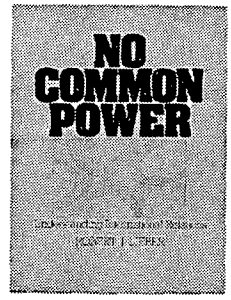

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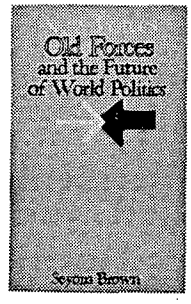

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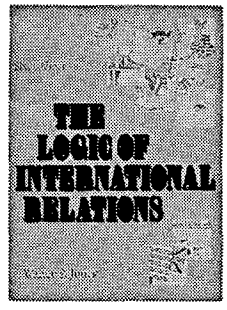

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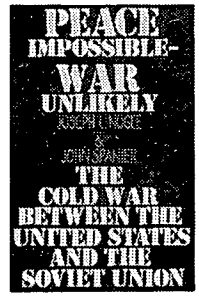

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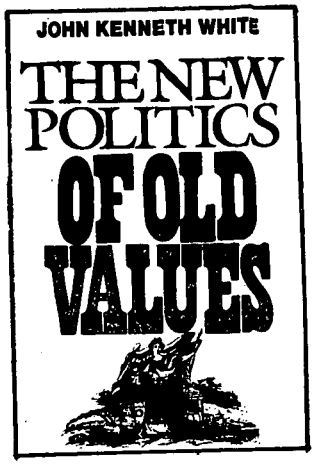

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CONTENTS

ARTICLES

One Soul at a Time: Political Science and Political Reform
Samuel P. Huntington 3

Structural Dependence of the State on Capital
Adam Przeworski & Michael Wallerstein 11

Max Weber's Liberalism for a Nietzschean World
Mark Warren 31

Political Science and the Enlightenment of Enthusiasm
James Farr 51

American Political Science, Liberalism, and the Invention of Political Theory
John G. Gunnell 71

Political Jurisprudence, the "New Institutionalism," and the Future of Public Law
Rogers M. Smith 89

Interest Group Politics and Economic Growth in the U.S. States
Virginia Gray & David Lowery 109

Institutions, the Economy, and the Dynamics of State Elections
John E. Chubb 133

Nuclear Brinkmanship with Two-Sided Incomplete Information
Robert Powell 155

Crisis Learning Games
Russell J. Leng 179

The Core and the Stability of Group Choice in Spatial Voting Games
Norman Schofield, Bernard Grofman, & Scott L. Feld 195

Principals, Bureaucrats, and Responsiveness in Clean Air Enforcements
B. Dan Wood 213

CONTROVERSY

Do U.S. Senators Moderate Strategically?
Robert A. Bernstein, Gerald C. Wright, Jr., & Michael Berkman 237

BOOK REVIEWS

Review Essays 253
Political Theory 261
American Politics 282
Comparative Politics 309
International Relations 335
Political Economy 351

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